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BRITISH MOSSES.

By the Rt. Hon. LORD JUSTICE FRY, F.R.S., F.S.A., F.L.S.

(*Continued from page* 25.)

IF the reader will now return to my table A, at the beginning of this article, he will see that I have given some account of all the Musci except the Anomaleæ; these are a somewhat heterogeneous group of plants, of great interest to the botanist, but with which I fear to detain my reader lest I should disgust him with apparently dry details.

Sphagnaceæ.—Next in order to the Musci in my table A will be found the Sphagnaceæ, or Turf or Peat Mosses, a natural group of comparatively few species and very marked organization. The general appearance of this class of Mosses may be gathered from the figure of one already given (see Fig. 16), and is well known to almost everyone who has had any interest in a hot-house.

Vast tracts of land in this country and throughout Northern Europe and America are covered with plants of this group, and large tracts which are now fertile agricultural land, where they have entirely ceased to grow, have in former times been occupied by them. The bogs of Ireland, which are mainly constituted of Turf Moss, were computed in 1819 by the Bog Commissioners to occupy 2,830,000 acres. No Moss has probably ever, at least in the present state of the globe, played so large a part as the Sphagnum or Turf Moss.

Structure.—It is to the peculiar structure of the Peat Moss that this great part on the theatre of the globe is to be attributed.

Leaves.—In the young leaves the component cells are all alike; then by a differential growth we are presented with large cells (sometimes of a square or rectangular shape) surrounded by narrower cells; then chlorophyll forms in these narrow cells, but is absent from the square cells; from these the contents disappear, and water or water-like fluid occupies the whole cell; subsequently annular and spiral threads develop on the walls of the square cells. The intimate structure of the leaf thus enables it to absorb great quantities of water.

But again, the shape of the leaves is in many species adapted to the retention of water. By a retardation of the lateral as compared with the mesial growth, the leaf assumes a boat shape. Often the edges of the leaves are turned over; the leaf thus affords means of holding water.

Figs. 27 and 28 will enable the reader to follow

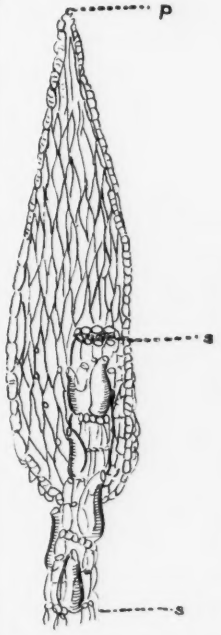

FIG. 27.—Leaf of *Sphagnum acutifolium*, magnified; *s s*, stem; *p*, point of the leaf. After Schimper.

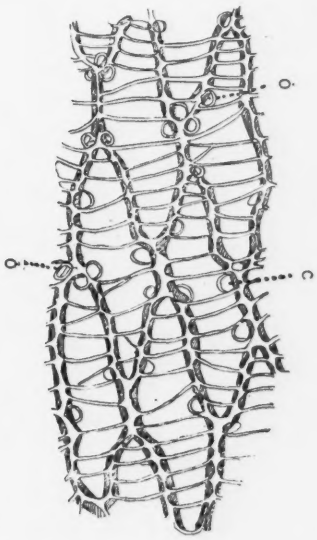

FIG. 28.—Portion of leaf of *Sphagnum acutifolium*, highly magnified; *o o*, orifices opening from interior of cells. After Schimper.

the foregoing description. Fig. 27 shows a magnified leaf of the *Sphagnum acutifolium*—with a portion of the adjoining stem (*s s*), of which more hereafter. The edges of the leaf are turned over—as may be seen by looking at the extreme point of the leaf (*p*) where these foldings over cease. Fig. 28 exhibits a portion of a leaf far more highly magnified; the large cells free from chlorophyll bounded by the narrower cells charged with it will be at once observed as well as the spiral threads developed on the walls of the larger cells, a peculiarity of the leaf of this genus which enables one to detect the presence of its remains so long as any organic structure is retained; and at the points *o o* are seen orifices opening from the interior of the cell and admitting water.

A reference to Fig. 16 will show that the leaves of the Sphagnum are borne on lateral branches. These at the

Fig. 29.—Lateral branches of *Sphagnum*; *s s*, stem; *h h*, horizontal branches; *p p*, pendant branches. After Schimper.

head of the plant form a thick and often widely extended tuft; but lower down the branches grow out laterally from the stem, generally in tufts of four branches, of which, as shown in Fig. 29, two generally grow out more or less horizontally, and two are disposed in close proximity to the stem, round which in fact they fall (see again Fig. 16), so as to exert a great capillary attraction and keep a great mass of water in suspension even against the force of gravity.

Yet further, as it would seem, to add to the absorptive capacity of the leaves, Nature, in one or more species of the Peat Moss, has recourse to a further expedient. Round the base and sides of the leaf, clusters of half free cells, with spirally marked walls, are clustered, ready, like their sister cells in the leaf, to carry their full complement of water. Such a group is shown (magnified of course) in Fig. 30.

The stem of the Sphagnum in like manner is developed as a water-carrying instrument. Its appearance when examined by the microscope, as shown in Fig. 27 and in Fig. 31, is very singular, for it is surrounded not only with large transparent cells of more ordinary shape, but with large cells developed into the shape of flasks, with openings at their tops. Fig. 32 will enable the reader further to realize this structure. It is a highly-magnified section of a quarter of a stem.

Again, the mode of growth of the plant, abandoning its moorings on the soil and throwing out roots into the water, and growing successively year after year, enables it not only to attain great growth, but also, when the occasion demands, to keep pace with the rise of the water in which it may be growing, "the individual thus becoming," it has been said, "in a manner immortal, and supplying a perpetual fund of decomposing vegetable matter."

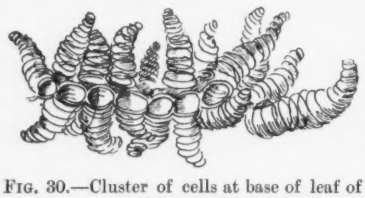

Fig. 30.—Cluster of cells at base of leaf of *Sphagnum acutifolium*. After Schimper.

Physical Results from Structure.—The result of these peculiarities is that the entire plant of any species of Sphagnum is a perfect sponge. When dry it is capable (as may easily be found by experiment) of rapidly absorbing moisture, and carrying it upwards through the plant; and when growing in vast beds it acts thus on a great scale. Everyone who knows Scotland must know how on many a steep mountain-side, or on the bottom and sides of a gorge, these beds will hold up a great body of water against the force of gravity; and again, the Irish bogs are described as often ascending from the edges towards the interior, sometimes by a gradual and sometimes by a sudden ascent, so that at times the bog is so high that it reaches the height of the church steeples of the adjoining country, without any rising ground intervening.

These peculiarities in the structure of Sphagnum have produced considerable physical effects.

(1) Everyone knows the different effects of rain falling on a land of bare rock or sand, like the Sinaitic desert, and on a porous soil. In the one case it produces a freshet or a flood, that leaves no trace behind; in the other it is held for a while in suspense, and only gradually passes into the streams. The glaciers and the Sphagnum beds of the mountains of Europe alike act as compensation reservoirs—receive large quantities of moisture as it falls, and retain it till the drier season comes, when part of it gradually passes away; but for these reservoirs, many of the rivers would exhibit a far greater shrinkage in summer and autumn than is now the case.

Fig. 31. — Stem of *Sphagnum molluscum*, magnified, showing *u u*, the utricles or flask-shaped cells. After Schimper.

But (2) the Sphagnum beds have become peat, and have gradually filled up the ancient lakes and morasses, and turned water into dry land. It is true that peat appears under some circumstances to be formed by other vegetables than Sphagnum, and in all cases it has probably some other plants or roots growing amongst it. Mr. Darwin tells us that in Terra del Fuego and the Chonos Archipelago, peat is formed by two phanerogamous plants, of which one at least seems endowed with an immortality something like that of the Sphagnum; and the peat of the fens of Lincolnshire is formed mainly of *Hypnum fluitans*. But Sphagnum appears to be the main constituent of peat in Ireland, Scotland, and, so far as my researches have gone, in England; the peculiar spiral threads of the cells of the Sphagnum leaf being easily detected in the peat so long as it retains traces of its organic origin.

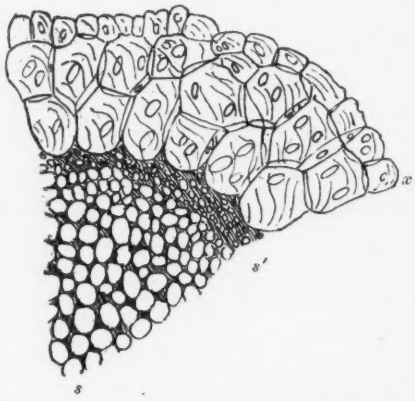

Fig. 32.—Magnified section of stem of *Sphagnum cymbifolium*. *s* s^1 stem; s^1 *x*, mass of spirally threaded cells surrounding stem. After Schimper.

Ancient Forests.—The Peat Mosses, and the sea-shores of our islands and of the adjoining mainland, reveal, as is very well known, traces of ancient forests. Many parts

of England, nearly all the mainland of Scotland, the Hebrides, the Orkneys, and the Shetlands, Ireland, and Denmark, the shores of both sides of the English Channel, Normandy, Brittany, the Channel Islands, and Holland, and the shores of Norway, all bear evidence to the presence of these primeval forests; and what is more, to the successive existence of forests, each in its order living above the buried remains of the earlier ones.

The following table will show the order of succession in the different species of trees in some of the places where this has been observed, the braces representing the co-existence of the trees:—

Island of Lewes.	Danes Moor, near Macclesfield.	Somerset.	Other parts of England.	Parts of Scotland.	Parts of Denmark.
1. Oak.	1. Scotch fir.	1. { Oak. Ash. Yew.	1. { Oak. Scotch fir	1. { Oak. Scotch fir	1. Scotch fir
2. Elder.	2. Larch.		2. { Birch. Hazel.	2. Birch.	2. Oak.
3. Birch.	3. Oak.		3. Alder.	3. Hazel.	3. Birch.
4. Scotch fir.	4. Birch.			4. Alder.	
	5. Hazel.			5. Willow.	
	6. Alder.			6. Ash.	
	7. Willow.			7. Juniper.	

In some Irish bogs fir, oak, and yew, and rarely elm, have been found.

What is the cause of the disappearance of these ancient forests one after the other? To this question various answers have been proposed.

The Romans, it has been suggested, in their inroads, cut ways through the forests and laid waste the land. But, wide as was the spread of the wings of the Roman eagle, the phenomenon in question is of far wider extension. They never conquered Denmark, or Norway, or Ireland, or the islands of Scotland: in Scotland, and even in England, their operations could never have covered the whole country; and as regards some of our Peat Mosses, we know that they must have existed long before the Roman invasion; for at least on the borders of Sedgmoor we have traces of their using peat for fuel as it is used there at the present day.

Still humbler agents have been invoked, in the supposition that the beaver and other rodents were the authors of the destruction of the forests. So far as I can judge, the cause suggested seems inadequate to the effect.

Again, changes in climate have been suggested. But, although there may be some evidence from the succession of the trees of a gradual amelioration in the climate, we know of no evidence of changes of so sudden and violent a character as would destroy the existing forests over large areas. Moreover, with few exceptions, the trees of the destroyed forests are such as are now found wild, or will grow easily in the spots where they lie buried.

The overthrow by storms has, again, been suggested as the cause of this wholesale destruction; and the fact that in some of the peat bogs of the west of Scotland the trees that have fallen lie to the north or north-east, and in some of those in Holland to the south-east, in the direction of the prevailing winds in those countries respectively, affords some reason to believe that wind has given the *coup de grâce* to the dying trees, and determined the direction of their fall. But it is much more likely that the work of the wind should be confined to this final overthrow of the decaying trees than that successive forests in full strength should have been swept from the face of vast tracts of Europe by the agency of wind alone. Moreover, in some cases the trunks as well as the bases and roots of the trees are found standing or buried in the bogs.

Allowing that some or all of these agencies may have had their part in the destruction of the forests, I believe that the growth of Sphagnum has been the greatest factor in the work of destruction. "To the chilling effect of the wet bog Mosses in their upward growth must be attributed," says Mr. James Geikie, "the overthrow of by far the greater portion of the buried timber in our peat bogs."

In a letter written by Lord Cromarty, in 1710, on Peat Mosses, and published in the twenty-seventh volume of the *Philosophical Transactions*, we get a curious account of the swallowing up of a forest by a peat bog. In 1651 the Earl saw in the parish of Lochburn (or, as Walker says, at Lock Broom, in West Ross), a plain with fir-trees standing on it, all without bark, and dead. Of the cause of their death he says nothing. Fifteen years after he found the whole place a Peat Moss or "fog," the trees swallowed up, and the moss so deep that in attempting to walk on it he sank in it up to his armpits.

This same process of destruction is still found to be going on in the mountain districts in the Harz and in Thuringia. "Forestry in these highlands," says Graf zu Solms Laubach in his *Fossil Botany*, "is everywhere at strife with the peat bogs, which, left to themselves, are always growing, and by the advance of their margins eat their way into the adjoining forests, and make irregular gaps in them."

(To be continued.)

THE LIFE OF AN ANT.—I.

By E. A. Butler.

THE way in which an Ant's nest originates is involved in some obscurity, and it is quite possible that the method is not always the same. Sir John Lubbock has shown that a nest may exist for years without the presence of a female, or "queen" as she would be called, and, on the other hand, if such be present, the number need not, as in the case of bees, be restricted to one. Though a nest which has once been established may continue to exist, at least for a time, without the intervention of any females, still it is most probable that for the origination of a *new* nest we must look to the initiative taken by a queen. The marriage-flight then being over, the young bride, or queen as she may now be called, even though she has no subjects to rule over, has apparently several courses open to her. She might, for example, return to the nest which produced her, or to some other already existent community, and contribute her share towards renewing or enlarging what is already well established. Or again, she might, if of a more independent turn of mind, get some stray workers to help her in founding a new colony of which she would be both the mother and ruler; or, thirdly, there is open to her, supposing her powers equal to the task, the possible

course of taking upon herself all the duties of maternity and colonization, and labouring with her own feet and jaws to prepare a home for, and to support her progeny till they are sufficiently advanced in age to take their proper share in life's burdens. There is scarcely sufficient direct evidence to warrant a decision as to which course would generally be adopted. Sir John Lubbock invariably found that when he introduced queens into queenless nests they were not accepted by the inhabitants, but were at once attacked with energy. Whether this antipathy was owing to a natural tendency, or was the result of the insects in question having been long unaccustomed to the society and rule of queens, is, however, doubtful. And since any eggs that may be laid by workers, a circumstance that sometimes happens, invariably produce males only, it would seem that a nest from which females are permanently excluded must sooner or later suffer extinction. Moreover, as showing that adoption into a strange nest may sometimes be the lot of the newly-hatched queen, McCook records such a case as having come under his own observation in America.

Sir John Lubbock's experiments in the direction of inducing females to rear their own young without help, and so start a new nest, were more successful. One day, in the middle of August, 1876, he found two pairs of the little red stinging Ant, *Myrmica ruginodis*, flying in his garden. He placed them quite apart from other examples of the same species, and provided them with all needful requisites in the shape of earth, food and water. All through the winter they remained alive and healthy—a somewhat unusual circumstance, as the males (Fig. 1) appear generally

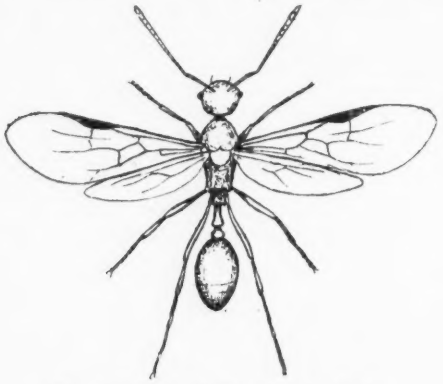

Fig. 1.—Male of *Myrmica ruginodis*. Magnified six diameters.

to die in the autumn. In this case they did not die till the spring, just about the time when their partners laid their first eggs. During the course of the summer various eggs were laid at different dates, many of which hatched in due course, passed safely through larvahood and pupahood, and at last reached the perfect state, coming out as workers some thirteen or fourteen weeks after the laying of the eggs. All attentions that they needed during this time were of course rendered by the parent queens, who thus proved that they had the power, if opportunity should call for its exercise, of founding new colonies. Other observers, however, experimenting with different species, have met with less satisfactory results, and it is of course possible that what one species can do in this respect another cannot. The same remark applies to their whole economy; for while the broad facts involved in their social habits are pretty much the same in all, yet no two species are exactly identical in habits, and one must guard against concluding that what is true of one kind is therefore necessarily true of another.

All Ants, of whatever species, commence their life as eggs —these are minute oval bodies, of whitish or yellowish colour. When laid, they have to be stowed away in suitable chambers excavated in the nest, and must be conveyed thither by the workers, by whom also they are carefully guarded. In carrying the eggs the mandibles are used, but to minimize the difficulty of transit, and the risk of damage, which would result from carrying such minute objects singly, they are caused to adhere together by their sticky surfaces, and can thus be conveyed away in batches. It is hardly necessary to point out that what are popularly called "Ants' eggs"—the large yellowish or cream-coloured oval objects which one often finds lying about in great numbers in the passages of the nest on removing the roof, and which are used as food for pheasants, singing birds, and fishes—are not the objects we are now speaking of, but are the pupæ of the Ants, which are looked after by the workers quite as assiduously as the true eggs. Their size alone is sufficient to check any such misconception, and to suggest their true character. But there are often to be found in Ants' nests batches of minute eggs, truly so called, which are not those of the Ants themselves, though guarded with as much care as their own. They are little dark-coloured objects, the eggs of aphides or plant-lice, which are kept by some Ants as domestic animals, and of which we shall have more to say in another paper.

From the egg is hatched, in a few weeks' time, a maggot-like grub (Fig. 2 A), whitish and semi-transparent, and

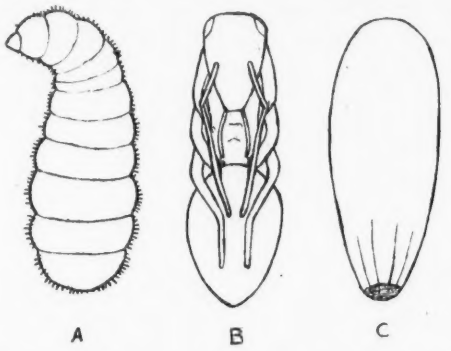

Fig. 2.—A. Larva. B. Pupa. C. Cocoon of Ant.

covered with short hairs; it is plainly divided into segments, but has no legs. It is rather conical in shape, tapering towards one end, at which is situated the mouth, furnished with a couple of tiny hooks, which, though suggestive of jaws, seem to be of little use as such. These larvæ are perfectly helpless, and cannot even feed themselves. Here is one of the penalties attendant upon the development of the social instinct; the insect is never left to itself, but has to be looked after and cared for from the day of its entry into the world as an egg till the time of its extrication from the last skin it will ever shed. In the case of a solitary insect, such as by far the greater number of insects are, no such care would as a rule be taken; the eggs would be left to themselves, and the larvæ would be capable of providing their own food, or if not actually doing so, yet of feeding themselves from a store previously accumulated by the mother. With the hatching of the eggs in an Ant's nest, therefore, the duties of the workers are enormously increased. Scores of little mouths have to be kept supplied with suitable food, each one several times a day, and but a brief intermission of their attentions

would probably be quickly attended with a fatal result. The food is elaborated by the nurses themselves in their own stomachs, and is supplied to the gaping mouths of the larvæ through their own mouths. Now as the eggs are not all laid at one time, there are frequent hatchings going on during the summer months, and in the same nest there will be grubs of different ages; these will of course require differing quantities of food, and possibly even food of varying quality, according to age and to the sex of the insects that are to result from them. Hence method has to be introduced into the management of the commissariat, and the nurslings are divided into classes according to their size.

But the young maggot-like Ant is a troublesome little creature, and needs a good deal more attention than what is involved in providing it with meals. Cleanliness is an important element in its education, and as it cannot clean itself, this duty also falls upon the nurses, which perform the necessary operations with their mouth organs. Just in the same way too the workers will clean one another, and Sir John Lubbock found that the Ants he had marked with spots of paint, so as to be recognizable again, had the spots in a little while removed by their friends, a delicate personal attention which suggests curious ideas of etiquette. For the attainment of the most healthy condition, a certain degree of warmth and moisture is necessary for the larvæ, and, as the various chambers in the nest will usually differ somewhat as regards these conditions, there results the necessity of carrying the babies about from place to place. The mandibles are again called into requisition for this purpose, and, like a cat carrying her kittens, the nurses gently take up the grubs in their jaws, and hurry with them along the galleries from nursery to nursery; if the sun shines, they will be taken to the topmost galleries, not indeed that they may be directly exposed to its rays, for this would apparently be injurious, but that they may be as near to its influence as the thickness of the covering stone or roof of the nest will permit. When the weather is cloudy, or when night comes, they must be carried down; or again, let but the nest be disturbed so as to admit the direct sunlight, and plenty of pairs of eager jaws are ready at a moment's notice to seize and hurry off below ground their fat and fleshy charges, until, in a few minutes, none are left exposed. Of course this may be quite as much for the sake of saving the treasures from the possible grip of the rash intruder, as to remove them from the influence of the direct sunlight.

The length of the larval life varies greatly. One of the common red Ants (*Myrmica ruginodis*) appears to be amongst the quickest in development, and in some of Sir John Lubbock's nests this species remained less than a month in the larval condition. On the other hand, some of the larvæ of the yellow Ant (*Lasius flavus*), viz., those of the autumn brood, are very tardy in their progress, and remain grubs throughout the winter, lying torpid in the deeper parts of the nest, destined not to complete their growth till the spring sunshine re-awakens the population of the nest, and supplies them again with energetic and painstaking attendants. What happens to the grub at the close of larvahood depends upon the species; all change into a chrysalis, but with a remarkable difference, the reason for which is not known. Those which when fully grown have not the power of stinging, as, for example, the common black Ant of the garden, envelope themselves in a silken cocoon, while those which do sting, such as the familiar red species before mentioned, have no such covering, but simply cast the larval skin and remain naked as pupæ. This rule, however, is not absolute, and Latreille discovered that the larvæ of one of our common dark-coloured stingless Ants, *Formica fusca* (Fig. 3) sometimes spin cocoons and sometimes do not. The pupa of an Ant (Fig. 3 B) is similar to that of a bee, but not like that of a moth, inasmuch as it exhibits distinctly the outline of the various parts and appendages of the future insect, such as the head, legs, antennæ, &c. But the whole insect is covered with a thin skin, which has to be removed when it reaches maturity before it can make any use of its perfected limbs. In the case of the stingless Ants then, this insect, enveloped in its thin skin, but with its various parts more or less distinctly revealed, is enclosed in its silken shroud like a mummy in a sarcophagus, the whole constituting a smooth oval body, with a dark dot at the end opposite that at which the head lies (Fig. 2 C). These are the familiar so-called "Ants' eggs" above mentioned, and they have to be carried about from one storey of the nest to another in the same way as the larvæ. In fact, the anxiety of the workers for the welfare of these objects is extraordinary, and those who have kept Ants' nests for observation have made use of this passionate devotion to their young, whether as larvæ or pupæ, to induce the Ants to travel in such directions as may be desired for the purposes of experiment, and the discovery of a store of larvæ or pupæ in any spot is to a worker Ant quite as strong an incentive to exertion, and quite as important a piece of news to be communicated to its fellows, as it would be to come unexpectedly upon an abundant supply of the most delicious food.

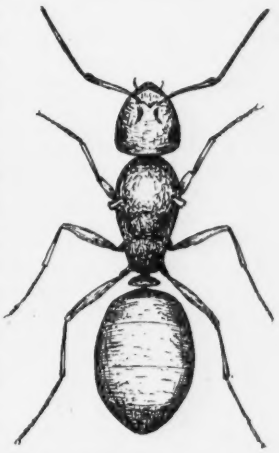

Fig. 3.—Queen of *Formica fusca*, showing remnants of wings.

The change to the chrysalis condition is, of course, not effected by the larvæ till after it has spun its cocoon, for when once it passes into this stage it becomes more helpless than ever before, and would, but for the assistance of the nurses, perish where it is and in direct consequence of its own act of walling itself round, and so cutting itself off from the world by a continuous and impenetrable barrier of silk. Here, again, appears strongly the helplessness of the individual member of the social community, as contrasted with the independence and power of the solitary insect. The caterpillars of many moths, as is well known, before turning into the chrysalis condition, surround themselves with a silken cocoon, which is sometimes, as in the case of the puss-moth, of so hard a consistency as to resist strong pressure, and to offer considerable opposition even to the entrance of the point of a knife. Within this covering lies the limbless pupa, apparently as effectually doomed to a lifelong imprisonment as any captive thrown into the old Bastille. And yet, without any assistance from outside, or any means other than what its own body supplies, the imprisoned moth first throws off its hard and crisp pupa skin, and then works its way through the walls of the cocoon, and after a little pause triumphantly proceeds, in its own unaided strength, to the business of its new life. Not so the Ant, however; for liberty, and indeed, for life itself, it is in many cases beholden to the same unremitting attention that has presided over its destinies hitherto. With a degree of intelligence which is truly remarkable, the nurses divine the right moment when the

imprisoned and invisible Ant is ready for release. Then, with their jaws they nibble and pinch at the loose-fitting investment and make a neat slit down one side, and thus open the prison doors. But even then the captive cannot avail itself of the liberty which is thus brought within its reach. It must be hauled out by the nurses, and stripped of the thin skin which still enshrouds its limbs; those limbs must then be helped into position, and the invalid stroked, caressed and fondled with antennæ and legs till it begins to collect its senses and become conscious of its powers. It must then be led and guided about the nest, till it is familiar with the details of that home in which, if its structure fits it for that purpose, it must now take its place as one of the great band of toilers, and show by its deeds that the care that has been expended upon its education has not been in vain.

Some three or four weeks will have been passed by the Ant in this state of inactivity, during which time it has taken no food. But when it enters on its perfect condition, hunger will again begin to assert itself, though the food taken will not, as heretofore, contribute to swell its bulk, for now waste and repair will be balanced, and the Ant has attained its full size. Amongst the workers there is often a good deal of variation in size; there are large ones which are called workers major, and small ones called workers minor, and often intermediate sizes also; but it must not be assumed that the minors will grow into majors; whatever size the worker Ant has on quitting the pupa skin, that it retains throughout the remainder of its life. The same applies to the males, and to the females as well, except in so far as the development of the eggs increases the size of the abdomen. The range of difference amongst the workers is not the same in all Ants. Take, for example, three of our commonest species; two of these, the yellow Ant (*Lasius flavus*) and the wood Ant (*Formica rufa*) show great inequality in size in the workers, while the common garden Ant (*Lasius niger*) has them much more uniform. The larger workers have often a proportionately larger head, indicating larger muscles and consequently more power in the jaws, whence it has been generally supposed that such forms are intended to act as soldiers and do the chief part of the fighting of the community, but pugnacity is by no means the monopoly of these big-headed forms, and the little workers are quite ready to do battle if occasion requires.

Up to the point to which we have now conducted it, our Ant has passed through a life of considerable monotony. Its most extensive journeys have not extended over more than a few inches of territory, and even these it has performed by the aid of others; its sole occupation, when it has been doing anything more than simply lying still and "developing," has been eating, and for this, too, it has had to be a pensioner on the bounty of friends. It has been from its birth an inhabitant of underground galleries and tunnels, and has never seen the light of day, except by accident. But now a vast change takes place; the whole world is before it, and if it is a worker, there awaits it a life, extending over years it may be, full of variety and activity, and crowded with incident and adventure. It is here that the social insect gets the advantage of the solitary one. The chief businesses that engage the attention of the latter are provisioning and love. The former is often uniform and prosaic enough, and even the latter, while it may involve a certain amount of incident and romance, is still as a rule an affair of such brevity that there is hardly time for anything very striking in the way of adventure to take place, before the adventurer is called upon to pay the debt of Nature. But with the development of the social instinct there comes an indefinite number of new responsibilities and endless opportunities of variety, resulting from the complexity of the life and the increased length of it, which seems to be the necessary accompaniment of the higher type of existence. To the threshold of this life of variety and incident we have now brought our Ant, and there we must leave it for the present, deferring till next month an account of its further adventures.

ELEPHANTS, RECENT AND EXTINCT.

By R. Lydekker, B.A. Cantab.

ASSUREDLY of all the Mammals now inhabiting this earth Elephants are those most justly entitled to the epithet "antediluvian," since they remind us, far more vividly than any of their modern contemporaries, of the gigantic extinct Mammals of various kinds which flourished in that latest epoch of geological history when man was but a comparatively new comer. A long acquaintance has, indeed, made us so familiar with the appearance of Elephants that we are too apt to forget what altogether strange and uncouth creatures they really are. If, however, they had happened to be included among those animals which disappeared from the face of the earth before the historic period, and were known to us solely by their skeletons, there can be no doubt that they would be regarded as among the most remarkable of Mammals. Moreover, if Elephants were only known to us by their skeletons it would be more than doubtful if we should ever have attained a correct idea of their true form; since, although the conformation of their jaws and teeth would clearly indicate that they must have had some very peculiar method of feeding, it would have required a very bold, not to say a very imaginative man to have conceived the idea that these creatures were furnished with that unique organ which we term the trunk or proboscis.

At the present day, it need scarcely be said, there are but two living species of Elephants, differing remarkably from one another not only in external characters, but also, as we shall notice later on, in the structure of their teeth; these two species being respectively confined to Africa, and to India and adjacent regions. These two kinds of Elephants are, however, merely the last survivors of a vast host of extinct forms, some of which were closely related to their living cousins; while others differed so markedly in the structure of their teeth as to have received the distinctive appellation of Mastodons, although they are really nothing but very generalized Elephants. These so-called Mastodons carry us backwards to the middle of that division of the Tertiary period of the earth's history known as the Miocene; but when we have reached to that stage all below is dark as regards the Elephantine pedigree. And it is, indeed, one of the most remarkable circumstances in Palæontology that although we know that Elephants belong to the great group of Hoofed or Ungulate Mammals, of which they form a well-marked division, yet we have practically no sort of knowledge of the many extinct forms which we presume must have connected them with Ungulates of a more ordinary type.

Although the trunk and tusks of Elephants form their most striking external features, yet it is not to these that the naturalist looks at first when enquiring into the true affinities and general structure of these animals, since these come under the category of specialized and acquired structures, which tell but little of an animal's past history; he looks rather to the structure of the internal skeleton,

which is always of especial value as being that part of the organism which is usually alone preserved in a fossil state. Let us then first turn our attention to the skeleton of these animals, of which we may see examples in our larger museums. The most remarkable feature noticeable in such a skeleton

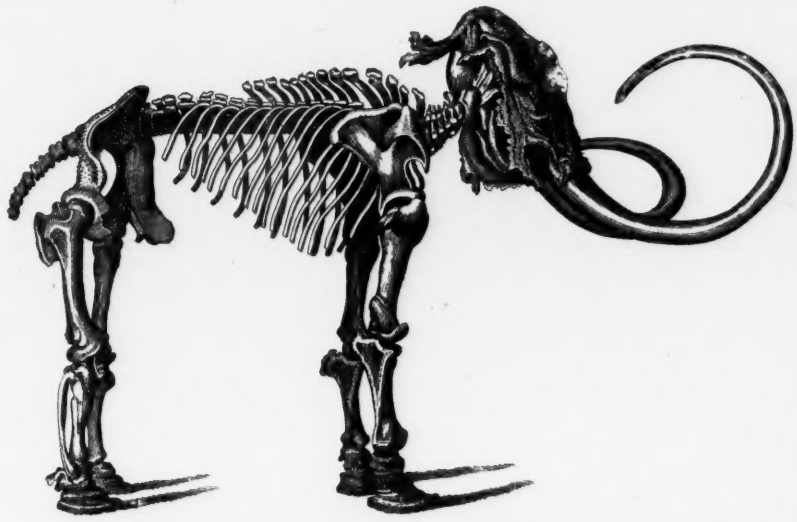

Fig. 1.—Skeleton of the Siberian Mammoth, with the skin still attached to the skull. *(From Jardine.)*

(Fig. 1) is that the various long-bones of the limbs are placed almost directly one above another, so as to form nearly vertical columns of support for the body; whereas in ordinary Ungulates, such as a horse or an ox, these bones are set very obliquely to one another. Moreover, as a similar vertical position of the limb bones occurs in several old extinct Ungulates which are known to be of extremely primitive organization, we may take it that an Elephant's limbs are likewise of a primitive type. We have, however, further evidence in confirmation of this primitive structure. Thus Elephants differ from all other living Ungulates in having five complete toes to all their feet (Fig. 2). Moreover, whereas in other living Ungulates (except the little Hyrax) the bones of the wrist are not situated in vertical rows immediately over the metacarpal bones of the foot, but, on the contrary, cross and overlap one another, in Elephants they have the former relation, with the single exception that the bone marked *l* overlaps the one lettered *td* to a certain extent. This difference may be illustrated by saying that if we were to take a hatchet and chop vertically upwards between the third and fourth toes of an Elephant's foot there would be nothing to resist the passage of the blade till it reached the bones of the leg, whereas in all other Ungulates—the pig, for instance — the blade could not pass through the wrist without cleaving solid bone. Again, whereas ordinary Ungulates walk solely on the tips of their toes, and are thus termed digitigrade, while the bones of the toes themselves are more or less elongated, Elephants walk on the soles of their feet in the so-called plantigrade fashion, and have very short toe bones. Now since all the large extinct Ungulates of the Lower Eocene or earliest Tertiary period also have five-toed feet, very similar to, but still shorter and of even simpler structure than those of Elephants, there can be no doubt as to the extremely primitive plan on which the entire limbs of the latter are constructed. As regards, therefore, its limbs and feet, an Elephant may be said to be an essentially old-fashioned animal.

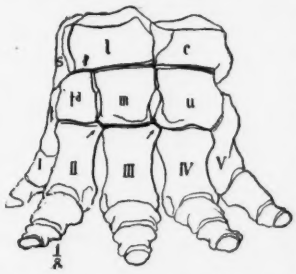

Fig. 2.—Bones of the left fore foot of an Elephant, $\frac{1}{8}$ natural size. The lettered bones are those of the wrist or carpus, and the numbered ones the metacarpals, below which are the bones of the toes.—*(After Osborn.)*

If, however, we turn to their teeth we shall find that Elephants are very far indeed from being of a primitive or old-fashioned type; the truth being that they are, on the contrary, very peculiar and specialized in this respect. The first and most obvious peculiarity in regard to their dentition is to be found in their tusks, which correspond to one of the pairs of upper front teeth in man, and also to the single pair of such teeth in the Rodents (rats, hares, &c). Moreover, these teeth, like the incisors of the Rodents, grow continuously throughout the life of the animal, owing to the circumstance that the pulp-cavity at their base always remains open, and has a permanent connection with the soft structures of the gum. In our own teeth, on the contrary, the pulp-cavity closes at a certain period, after which there is a total cessation of growth. These ever-growing tusks of the Elephant are preceded in the young animal by a pair of small milk-tusks, with a closed pulp-cavity, which are shed at an early period of life. In both of the living species of Elephant the tusks are confined to the upper jaw; but whereas they occur in both sexes in the African Elephant, in the Indian species large permanent tusks are restricted to the male, and are not, indeed, invariably present in all individuals of that sex. The primitive Elephants, or Mastodons, frequently, however, had tusks in both the upper and lower jaws; and since these did not generally attain the huge dimensions which they reach in many true Elephants, it is evident that in this respect the Mastodons departed less from the ordinary type of Mammals, where the front teeth are not greatly larger than the hinder ones, and those of the upper and lower jaws correspond with one another in size and number. Before leaving the subject of tusks, it may be mentioned that the ivory of which they are composed differs from the so-called ivory of other teeth in a manner which renders it always easy to determine whether a reputed ivory article is genuine. This peculiarity consists in the circumstance that a transverse section of a tusk exhibits a series of fine, decussating, curved lines radiating from the centre to the circumference, and forming curvilinear lozenges at their intersections. This peculiar structure is in fact precisely similar to the "engine-turning" on the back of a watch; and in an ivory knife-handle it should be distinctly visible at the butt. *(To be continued.)*

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THE MOVEMENTS OF THE STARS.

By Miss A. M. Clerke.

PROPER motions may be described as the individual, apparent, angular displacements of the stars. They are small residual irregularities, becoming persistently manifest year by year, or century by century, after all the usual systematic corrections have been applied, through which star-places of various dates are rendered strictly comparable. And because they are individual, they can only be ascertained by observation. No process of reasoning is available by which, from the known movements of nine hundred and ninety-nine stars, the unknown movement of a thousandth star can be calculated. There may be a logic by which the feat could be accomplished, but we are absolutely ignorant of its rules. Proper motions are then experiential data, defying formulaic expression, which have to be tabulated one by one—each as a fact apart. Some thousands of them are now, with very fair accuracy, disposable by astronomers; and their registration has been no trifling achievement. Yet they are only the unshaped stones out of which the edifice of knowledge regarding sidereal structure has to be built: or say rather, the pile of clay collected antecedently to the moulding and baking of bricks for that high purpose.

It is true that mere empirical acquaintance with proper motions serves all the purposes of practical astronomy, since it confers the power of predicting, for an indefinite time to come, the places on the sphere of the stars they affect. But the physical astronomer has other ends in view. For him the "sphere" has no more reality than the crystalline barriers fencing off one from the other the successive heavens of the ancients; he regards it as an ideal canvas, upon which the signs and wonders of the sky are pictured. His true concern is with the ocean of space, and the voyages amid its depths of the fiery craft everywhere furrowing them.

The direction and speed, however, of these voyages are only in part and imperfectly indicated by telescopically measured proper motions. No determinate results, in the physical sense, can be elicited from them until they have first been combined with further items of information of three several kinds. For they include, to begin with, a common perspective element due to the translation of the solar system. This must be eliminated as a preliminary to assigning the "peculiar" movement due to each star as a body traversing space on its own account. Secondly, proper motions are angular amounts only. To render them linear, we should be acquainted with the distances from ourselves of the objects displaced by them. Thirdly, proper motions are the projections upon an imaginary plane of lines of travel forming unknown angles with that plane. Only that portion of stellar movement which lies across the visual ray is represented in them; they tell nothing about the component along the visual ray.

None of these three requisites for deducing individual real motions from proper motions is indeed entirely wanting even now; and all are likely to be more completely provided in the future. A thoroughly satisfactory determination of the sun's course and velocity will evidently before long be accomplished through the accumulation of precisely known stellar radial movements; and by the same means, the missing component of proper motions first cleared of the effects of our own interstellar journey, can already be supplied in some cases, and will before long, be supplied in many more. The list of measured star-parallaxes, too, lengthens continually; yet here it must be admitted that the prospect of improvement is less assured than elsewhere. For relative parallaxes only are given by the methods at present in use, and doubts of the gravest kind as to the validity of conclusions from relative to absolute parallax are beginning to force themselves upon astronomical attention. It is, nevertheless, consolatory to reflect that misgivings as to the genuineness of the results so far attained in this important branch of research are strongest where sensibly fixed objects are in question. Stars with appreciable proper motions detach themselves from their background, and so afford a satisfactory criterion for the choice of trustworthy reference-stars. Hence the parallaxes of swift stars can rarely be wholly illusory, and are probably, for the most part, but very slightly underestimated.

In the meantime, what conclusions can be derived from the facts actually before us? Do they provide ground for even a rational surmise as to the dynamical relations of the stars? At the threshold of the enquiry we are startled by the phenomena of what are called "runaway stars." Of these, Groombridge 1830, with its thwartwise velocity of 232 miles a second, is the classical example; but it is outdone by Arcturus with 375, and by μ Cassiopeiæ with 305 miles a second; and several stars besides—if the small parallaxes attributed to them can be depended upon—shoot through space at rates varying from 70 to upwards of 100 miles a second. Twelve, moreover, out of 52 stars with numerically valued tangential velocities, progress at a speed exceeding 50 miles a second. Stars, then, of the "flying" description, are no great rarities. Among them are to be found both single and compound objects, enormous bodies like Arcturus, and orbs on the modest solar scale, such as Groombridge 1830; and they show spectra of sundry varieties. Some are probably highly luminous in proportion to their mass, others give little light, while exercising a strong mutual attraction. Of the latter sort, at least, are the semi-obscure revolving pair carried with it by o^2 Eridani in its particularly well authenticated advance of 71 miles a second. The swiftest stars cannot, accordingly, be regarded as forming a class by themselves; their velocities, although unaccountable, and, by calculable gravitational power, uncontrollable, are evidently systematic, that is, belong to the settled order of sidereal arrangements, and have to be dealt with in any attempt to grapple with the problem of sidereal mechanism.

No radial velocities at all comparable with the high tangential velocities of late pretty freely disclosed, have yet been detected. Aldebaran, with 30 miles a second of recession from the sun, bears the palm among 47 stars spectrographically determined at Potsdam, and their average rate comes out no more than 10·6 English miles. But the average value of the visible component for 52 stars with ostensible parallaxes,* is 42 miles, the effects of the solar motion being of course impartially mixed up with both kinds of result. The tangential rate, however, since it is measured along great circles of the sphere, sums up motion in the two co-ordinates of right ascension and declination, the separate value of each of which, amounting to just 30 miles a second, is the quantity properly to be contrasted with the figure derived from the Potsdam observations.

A striking disparity, none the less, still remains, and suggests curious reflections; although no great stress can be laid upon them until it be seen whether the anomaly giving rise to them be abolished or accentuated by further research. Possibly it may be due to the character of the objects examined for two different purposes. The Potsdam

* One of them is Arcturus, which can scarcely be said to have even an "ostensible" parallax, since its probable error considerably exceeds its nominal amount. The star should rather be called indefinitely remote.

list comprises mainly stars of the second magnitude, which, for some unknown reason, possess exceptionally slow visible motions; and it excludes those faintly-luminous, swift objects so frequently chosen as the subjects of experiments for parallax. Hence, when spectrographic measurements are extended to stars of the sixth and seventh magnitudes, radial motions of hundreds of miles a second may be brought to light. There seems no good reason why they should not exist; for in our present ignorance as to the dynamical relations of the stellar crowd, we cannot with any confidence anticipate the display by its members of preferences in the shaping of their courses. Arcturus, then, which chances to have nearly the whole of its tremendous speed directed *across* the line of sight, may prove to have opposite counterparts in stars telescopically fixed, but rushing with scarcely less rapid *end-on* motions towards, or away from the earth. The discovery, however, of such hypothetical objects will demand perseverance or good fortune; since no suspicion can be raised beforehand of a peculiarity betrayed by no visible symptom.

If indeed there were reason to believe the stars combined into a stable system, and wheeling unanimously round a common centre, then their tangential might be expected to prevail very greatly over their radial velocities, as measured from a point anywhere in the neighbourhood of that centre. Thus, the planetary movements would lie nearly altogether across the line of sight of an observer on Mercury; and the movements in the Saturnian system would be similarly related to one stationed on Mimas. But the plan of stellar organization bears no traceable resemblance to that followed in the planetary and Saturnian systems; so the argument from analogy falls to the ground. Still, it is important to recall that the postulate of approximate equality between the average values of different components of stellar velocity, besides being flatly contradicted by our present, certainly most imperfect, experience, really involves some kind of theory, or negation of a theory, regarding sidereal construction.

This postulate has lately served for the foundation of an attempt to solve the problem of mean stellar distance. Dr. Kleiber (*Astr. Nach.*, No. 3037) laid it down as a principle not likely to be disputed, that, in a sufficient number of casually distributed velocities, the sum of movement projected in any given direction cannot differ very appreciably from the sum of movement projected in any other; and proceeded to compare the radial speed of 22 stars measured by Professor Vogel with their proper motions as determined by Dr. Auwers. The sum of these last proved to be, in right ascension 4·83″ (after due multiplication by the cosine of the declination), in declination 5·51″; while the sum of the corresponding end-on movements was 363 kilometres per second. Hence, taking an arc of 4·83″ to stand for so many kilometres as result from multiplying 363 by the number of seconds in a year (31,556,929), the mean parallax of the 22 stars comes out 0·065″, implying a light journey of 50 years; the proper movements in declination giving similarly a parallax of 0·074″ (light journey=44 years). The mean magnitude of the stars made the subjects of this interesting, if not entirely convincing, experiment is 1·8; so that its upshot is in plausible agreement with Dr. Elkin's directly measured mean parallax of 0·089″ for 10 stars of the first magnitude.

The real significance of stellar movements has yet to be penetrated. Setting aside the accordant impress stamped upon them by the sun's onward march, they seem almost purely erratic. Certainly they obey no obvious stream of tendency. They are executed indifferently in all planes, and show no methodical graduation of velocities. They disregard the lie of the Milky Way, and refuse to allow a "rotation-component" with reference to it to be elicited from them. Shall we then give ear to a "counsel of despair," and assert that the "mighty maze" is "without a plan"? This would surely be rash; despair always is rash. Millenniums indeed are but as moments in the development of the harmonizing idea which we expect to lay hold of and apprehend in the course of a few generations of ephemeral existence. But that such an idea is present—that the stars do not cruise at random and rudderless, drifting at the mercy of wind and tide, so to speak, through space—a very little consideration suffices to show.

The case, on a general view, seems to stand thus. The recorded observations of stellar movements all, or nearly all—for radial measurements are independent of distance—refer to stars comparatively near the sun. These are promiscuously distributed throughout the vast region enclosed by the annulus of the Milky Way, yet after a manner not wholly irrespective of its structure. They show, on the contrary, decided condensation towards the plane of that stupendous collection, and form with it, undoubtedly, one highly complex aggregation. No aggregation of moving bodies can, however, continue to exist apart from their subjection to some governing law. "Bees in a swarm," for instance—to adopt Professor Young's chosen example—would very quickly cease from that condition unless maintained in it by an efficacious "clustering power." Their courses must be more or less pendulous, if not centrally deflected. A few seconds of persistent flight along straight lines would bring about the disintegration of the little community. And so, just because the sidereal aggregation is a fundamental reality, and not the casual product of an eternal and meaningless flux of things, star-movements must be controlled by some primary and overmastering force, acting uniformly, universally, and irresistibly. Did they truly possess the lawless and insubordinate character sometimes imputed to them, we should be compelled to regard the constellated suns of our midnight skies as mere adventurers from the void brought into temporary companionship within the circuit of the galaxy, and obeying only King Arthur's law of life—

> "From the great deep to the great deep they go."

But this is simply incredible.

[A general aggregation of the brighter stars towards the zone of the Milky Way has long been recognized. Dr. Gould has carefully studied the symmetry in the arrangement of the brighter stars, and points out (in the *Uranometria Argentina*, p. 361), that the brighter stars are more uniformly distributed with respect to the medial plane of the zone of great stars (described in the article on the Pleiades cluster in KNOWLEDGE for May, 1891) than with respect to the medial plane of the Milky Way. The zone of large stars follows a great circle round the heavens, making an angle of about 20° with the medial plane of the Milky Way.

It does not follow, from the general symmetry in the arrangement of the brighter stars which is observable, that all the brighter stars belong to the Galactic system, or that they are connected with the great zone above referred to. A third or even a half of their number may be distributed at random over the sphere. The chance distribution of a certain number of stars would not obliterate any symmetry that might be evident in the arrangement of the stars of the Galactic cluster.

Some twelve years ago Professor Simon Newcomb showed that stars having a velocity in space of 25 miles a second could not be controlled by the gravitation of the Milky Way system, and that such swift stars could not therefore be

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permanently associated with it. The assumptions made use of by Professor Simon Newcomb are probably far too liberal (see *Old and New Astronomy*, part XII.). Our own sun and even stars having a velocity of 15 miles a second must probably be assumed not to belong to the Galactic system. It is a curious fact that stars with the solar type of spectrum have on the average large proper motions compared with stars giving the Sirian type of spectrum, and that Sirian stars cluster thickly towards the Milky Way and the great star zone—while Solar stars seem to be much more irregularly distributed. It points to the conclusion that the Galactic system is a cluster of Sirian stars associated with nebulæ through which the sun happens at present to be passing.—A. C. Ranyard.]

THE MOON'S ATMOSPHERE.

By A. C. Ranyard.

If the Moon had a copious atmosphere at all like our own the parts of the Moon near its limb or outer smooth edge would appear reddish and decidedly less bright than the central portions, for the limb of the Moon would be seen through a great depth of the lunar atmosphere, and the light of the limb would be reduced just as the light of our sun is reduced at rising or setting as compared with the light of the mid-day sun. But the limb of the Moon is not in the least dimmed or hazy.

In fact, it is always one of the brightest parts of the Moon's disc, and is very sharply defined, as will be seen by examining the plate published with this number, which is made from one of the beautiful photographs of the Moon taken by the Brothers Henry, and if the Moon's atmosphere were at all like our own we should expect to see a sensible zone of twilight along the Moon's rough edge, but the shadows between the illuminated mountain tops appear on the photographs and to the eye perfectly black. There is no recognizable refraction when the Moon comes between us and a more distant object, such as the sun, during an eclipse, when we should expect an atmosphere to cause a very evident distortion of the sun's limb where the Moon cuts it, and there is no recognizable prolongation of the cups of the thin solar crescent into a narrow ring of light such as was observable round Venus just before it entered upon the sun's limb.

But several observers of partial eclipses of the sun have described a band or shade on the sun just outside the Moon's limb. It cannot amount to a very notable darkening of the sun's disc, for all observers do not notice it; some have looked for it and have not been able to detect anything of the kind. On the other hand, many photographs of the partial phases show a bright band on the sun's surface running along the outside of the dark Moon's disc. This brighter band on partial-phase eclipse photographs was at first thought to be due to an effect of contrast, but its actual existence as a band of denser photographic action on photographs taken during the eclipse of August 7, 1869, in America, was demonstrated by Dr. Edward Curtis, who showed that the photographic action produced by the sun's light was denser close to the Moon's limb than at a little distance from the limb; so that printed dots placed behind the negatives were hidden by the brighter band adjacent to the Moon's limb, while they could be distinctly seen through the photograph of the sun's disc at a little greater distance from the Moon's limb.

Photographic plates often show differences of brightness of different parts of the sun's disc which are not recognizable by the eye; thus a not too much exposed photograph of the whole sun's disc generally appears much fainter towards the limb, while the eye hardly notices the greater brightness of the sun's central parts as compared with those near the edge of the disc. A band of *increased* density might, it is suggested, be caused in an over-exposed photograph by a part of the sun's disc being dimmed down by a lunar atmosphere, so that the well-known reversing action (which after a certain limit of exposure begins to take effect) goes further on the brighter regions of the sun's disc, away from the lunar limb, than on the region close to the lunar limb.

The earth's shadow through which the Moon passes during a lunar eclipse is always found to have a slightly greater diameter than the geometrical shadow calculated from the known diameters of the sun and earth. The excess of the diameter of the observed shadow region over the calculated diameter of the geometrical shadow is generally put down at about one-sixtieth, that is, the earth casts a shadow as if its atmosphere to a height of about 66 miles above the sea-level acted as an opaque rind or covering through which the sun's light cannot pass without having its intensity materially diminished.

The Earl of Rosse and Dr. Boeddicker, in their account of observations of the Moon's radiant heat* observed during a total lunar eclipse, came to a somewhat similar conclusion with regard to the heat-absorbing power of the earth's atmosphere, which they believe is recognizable with their great 3-foot telescope and apparatus to a height of not less than 190 miles above the earth's surface.

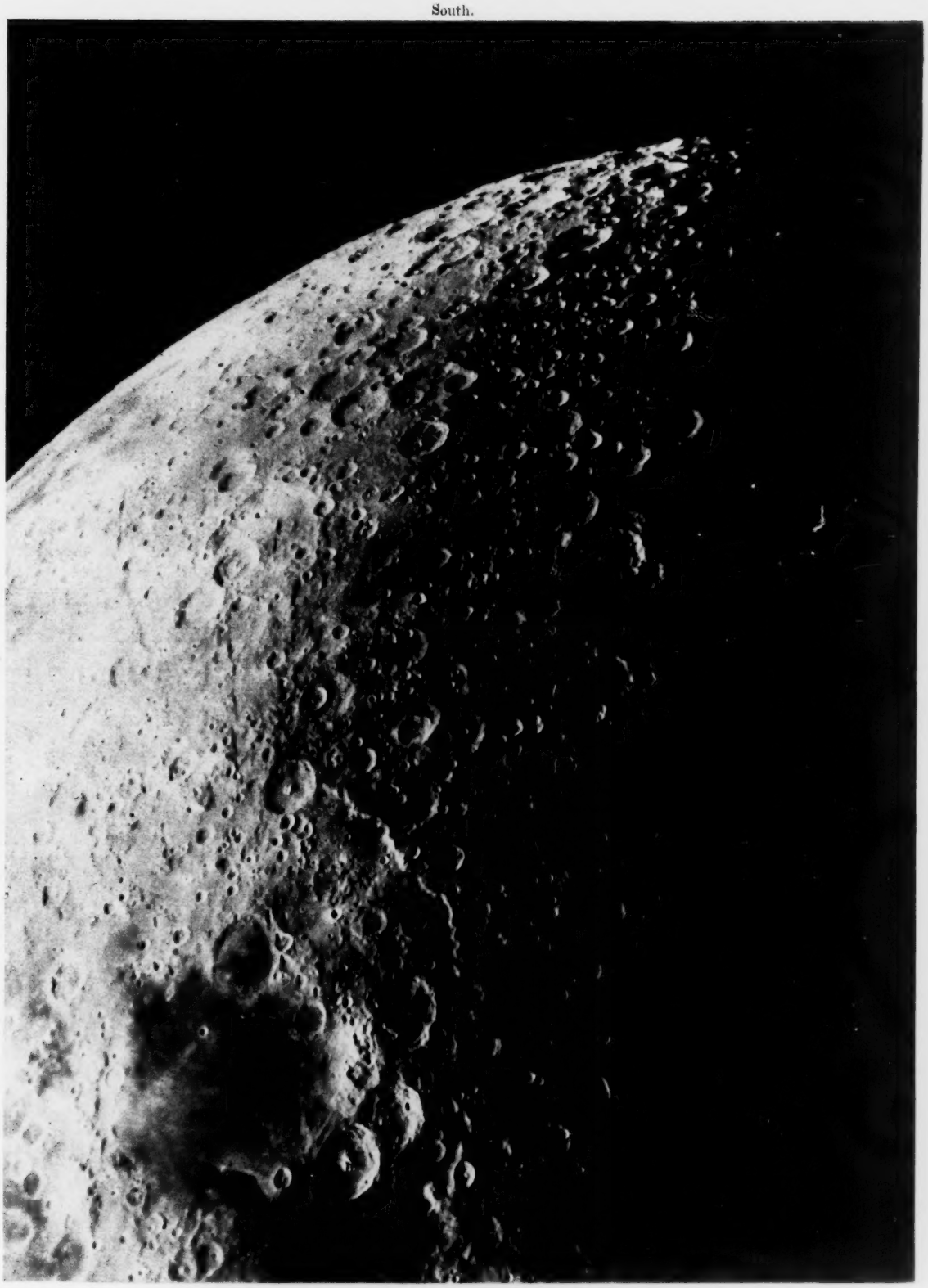
The measures of the diameter of the shadow of the earth through which the Moon passes during a lunar eclipse are necessarily very indefinite, but we may probably assume that the Moon has not an atmosphere which corresponds in light-absorbing properties to the earth's atmosphere at a height of 50 miles above the sea-level, that is (according to the law of decrease of density stated in the article on the Earth's Atmosphere in Knowledge for November last), the Moon probably has not an atmosphere which is $\frac{1}{200000}$th part as dense as our own atmosphere at the sea-level. But our atmosphere, if it were transferred to the Moon, would only be about one-sixth as dense at the Moon's surface as it is at the earth's surface, for gravity at the Moon's surface is only about one sixth as intense as terrestrial gravity at the earth's surface, and though the atmosphere would continue to support about 30 inches of mercury at the lunar surface, both the mercury and the atmosphere would have their weight reduced to one sixth of their terrestrial weights. Consequently the atmosphere at the lunar surface being compressed by only one sixth of the weight which it is compressed by here would occupy six times the volume it occupies here, and it would be one sixth as dense as the atmosphere at the earth's surface, and neglecting differences of temperature, the whole lunar atmosphere would extend six times as high as the earth's atmosphere—so that if, in pursuance of the reasoning of the article in the November number, we assume that the earth's atmosphere does not extend to a height of 250 miles above the earth's surface, the lunar atmosphere would not extend to a height of 1500 miles above the lunar surface.

Let us now follow the Moon backwards in time according to Prof. Geo. Darwin's assumption, until it approached Roche's limit, where it would be torn to pieces by the tide produced in it by the earth's attraction and broken up into a ring, say (assuming the

* Published in the Scientific Transactions of the Royal Dublin Society, 1891.

South.

West.



East.

PHOTOGRAPH OF THE MOON.

Taken by MM. PAUL and PROSPER HENRY with their 13-inch refractor, at the Paris Observatory. The sensitive plate was placed behind an eye-piece which enlarged the image in the principal focus 15 diameters. Age of the Moon 167 hours. Taken March 27th, 1890.

The Automatic Engraving Company, Strode Road, Willesden Green.

XUM

earth and Moon to have their present diameters and to be homogeneous bodies) when the Moon was at a distance of 10,000 miles from the earth's centre.

Until Roche's limit was approached the summits of the terrestrial and lunar atmospheres would be widely separated by a gulf of more than 3000 miles, and unless one or both bodies were very hot, and the heights of their atmospheres were greatly raised by temperature, we cannot suppose that there would be any intermixture of the atmospheres and consequent drafting of gas from one body to the other. If, however, they were both in a sun-like condition and their atmospheres or coronas intermixed, we should have an exchange of gaseous matter going on.

If they were both about the same temperature, the smaller body having the least gravitating power, would have the highest atmosphere, and a transfer of gaseous matter would go on from the smaller body to the larger body. If, however, the smaller body cooled first and the larger remained in the sun-like condition, we might have a transfer of gaseous matter from the larger to the smaller body, owing to a condensation of vapour on the side of the smaller body remote from the larger. Such exchanges may possibly account for the different density of the earth and the Moon,* and also, as I have suggested in another place, for the different colours of binary stars, and the curious fact that the smaller star of a binary is apparently always bluer than the larger star.

STELLAR SPECTRA.

To the Editor of KNOWLEDGE.

SIR,—In KNOWLEDGE for June, 1891, Mr. Maunder announced, for I believe the first time, a principle as to the relative brightness of stars with different spectra, which seems destined to bear very important fruits in Astronomy. I have lately compared the stars treated of by him with the Draper Catalogue of the Harvard University, with the result of removing most of the anomalies in his table, save that of the extraordinary brilliancy of γ Leonis. Four stars with spectra of the first type (Sirians) in Mr. Maunder's list, have a relative brightness of less than 4·0. These are O Σ 4, relative brightness 1·48, ξ Cancri, relative brightness 2·90, η Coronæ Borealis, relative brightness 1·40, and μ^2 Bootis, relative brightness 2·74. Mr. Gore (whose figures Mr. Maunder follows) seems to have under-rated the magnitude of the first of these stars. Its photographic magnitude is 6·46 according to the Draper Catalogue, if I am right in my identification, and I find that even with Sirian stars the photographic magnitude is usually less than the photometric. Taking its magnitude at 6·46 its relative brightness is 5·53 instead of 1·48. Otherwise it is not in the Catalogue. ξ Cancri, according to the Draper Catalogue, has a spectrum of the Solar not the Sirian type, and the same remark applies to η Coronæ Borealis. μ^2 Bootis is not in the catalogue at all. The star with a spectrum of the first type is μ Bootis, a brighter star which is situated near the binary pair, and I suspect that it was the spectrum of this star that Mr. Gore inserted in his table. 4·00 thus seems to be the minimum of relative brightness for a Sirian star, if indeed any of them sink so low, while there are only five solar stars in Mr. Maunder's table (besides γ Leonis) which rise so high. The brightest of these is π Cephei 11·07, the spectrum of which is marked with a ? in the Draper Catalogue, and from a note it appears that on special examination the spectrum was considered to be of the *third* type. That stars of this type should be relatively brighter than those of the second type (Solar stars) is no doubt an unexpected result, but it is confirmed by the next case, 36 Andromedæ 6·23, whose spectrum is also referred to the third type in the Draper Catalogue. τ Ophiuchi 7·35 is the brightest of the remainder, Doberck's orbit being adopted. As different computers have differed considerably with regard to the period of this star, I think the orbit would be worth re-computing, including recent measurements. The relative brightness of the remaining two is under 5·00. I may note that several stars classed by Mr. Gore and Mr. Maunder as Sirian, appear as Solar stars (spectrum F or E) in the Draper Catalogue. Their spectra seem to lie on the border-land, and Pickering gives a wider extension to the second type of spectra than to the first. But these stars confirm the general conclusion drawn by Mr. Maunder, for their relative brightness is in all cases below the Sirian average and above the Solar average. Their intermediate position is thus established.

I remain, your obedient Servant,

January 5th, 1892. W. H. S. MONCK.

P.S.—I find that I was in error in stating that the photographic magnitudes are usually less than the photometric in the case of stars with spectra of the first type. With the brighter stars they are almost invariably less, but with the fainter stars they are often greater. It is, in fact, clear that the two scales do not proceed on a common basis. W. H. S. M.

* The earth's density being taken as unity the Moon's density is 0·613.

THE INHERENT LIGHT OF JUPITER.

To the Editor of KNOWLEDGE.

SIR,—Among the "other considerations" referred to by Mr. Gore in his very interesting article (on page 36), whence the self-luminosity of Jupiter may be inferred, perhaps I may be allowed to recall those derivable from an observation of my own made in 1880, of which the details will be found on page 47 of vol. XLI. of the R.A.S. *Monthly Notices*. On the occasion to which I refer the shadow of Satellite II. was distinctly of a chocolate colour; and the only feasible explanation that I could find of this phenomenon was, that the portion of the planet upon which the shadow was projected must have been red-hot. If we were to illuminate a whitewashed wall by light from a lamp or lantern transmitted through a red glass, obviously the wall would appear red. If now, we flooded it with the electric light, this would wholly overpower the more feeble illumination, and the wall would appear white. It is evident, though, that any object interposed between the electric light and the wall would cut off the more brilliant form of illumination, leaving the feebler one to assert itself; and that hence shadows would appear not black but red. I may add that Mr. Campbell, F.R.A.S., observed this brown shadow of Jupiter's second satellite at Barnet, simultaneously with my own observation of it in Sussex.

I am, faithfully yours,

Forest Lodge, Maresfield, Sussex, WILLIAM NOBLE
February 2nd, 1892.

[The intensity of the inherent light of Jupiter must be small as compared with the brightness of the sun's light at the distance of Jupiter, for the satellites are entirely lost to our view when they plunge into the shadow of Jupiter. If their brightness were reduced only eight stellar magnitudes when they plunged into the shadow of the planet they would still easily be visible in large telescopes. For the third satellite, which is the brightest, is usually estimated as equivalent in brightness to a star of the sixth magnitude. Therefore, we may assert that the illumination derived from the whole disc of Jupiter (which subtends an angle of nearly 19°, as seen from the nearest

satellite) is not one fifteen-hundredth part as great as their solar illumination, or the illumination derived from the sun, which subtends an angle of less than 6′ as seen from the distance of Jupiter's satellites, must be more than 1500 times as great as that derived from the intrinsic luminosity of the planet. I am therefore inclined to think that Capt. Noble's observation must probably be explained as due to contrast or some other physiological cause. I have been frequently struck by the amount of colour visible on the disc of Jupiter as seen in an 18-inch silver-on-glass reflector. To my eye the body of Jupiter shows warm tints of red, canary colour, and blue, which are very noticeable directly the attention is directed to the colouring.—A. C. Ranyard.]

[Having been kindly favoured by the editor with an opportunity of reading the above note, I would merely say that the tints of which he speaks on Jupiter have their ultimate origin in sunlight, as much as the red colour of a holly berry or the yellow of a primrose has; and that, hence, such tints would be wholly invisible were the sunlight cut off from the surface emitting them by the shadow of a satellite. I perhaps might have added to my original letter, that, during the visibility of the chocolate-coloured shadow of Satellite II., that of Satellite I. was seen on another part of the planet's disc, like a small circular spot of the blackest ink. The fact that Mr. Campbell simultaneously saw (absolutely independently) the shadow of the second satellite of the same colour as I did, surely negatives any subjective cause of the phenomenon. A physiological cause must, so to speak, be idiosyncratic.—W. Noble.]

To the Editor of Knowledge.

Sir,—Having, during the past six years, devoted my Observatory to the investigation of the light curves of variable stars, I venture to draw the attention of your readers to the following phenomena which I have not seen noticed elsewhere, except in the case of the Nova Aurigæ, which was described in *The Times*, 3rd inst., as "slightly fuzzy" when first observed:—

I have given below extracts from my note book with regard to three stars, and have, with my assistant, Mr. Grover, frequently observed—

(*a*) A remarkably well-defined, almost planetary, disc.

(*b*) A well-defined star, surrounded by a more or less dense, ruddy atmosphere.

(*c*) A large, woolly, ill-defined image, resembling a small but bright planetary nebula.

(*d*) At minimum, in place of the variable, a slight bluish nebulosity.

I do not consider that I have a sufficient series of observations on which to base any theory, but trust that other observers may be induced to take up what appears to me to be an important field of enquiry. The telescope in use is an achromatic of $6\frac{1}{4}$ inches aperture.

Yours faithfully,

Rousdon Observatory, Devon, Cuthbert E. Peek.
February 17th, 1892.

T. Cassiopeiæ. 1889, April 29th. 7·9 mag. Very red, surrounded by a ruddy haze in striking contrast to the clear white of No. 6 (another star in the field). September 10th. 7·7 mag. Very red. A well-defined star shining through a ruddy haze ("a" night; moonlight). September 18th. 7·9 mag. Very red and hazy. ("a"* night). 1890, March 9th. 9·9 mag. Very deep red, ill-defined. ("a" night.) October 16th. 6·6 mag. Large and ill-defined. ("a" night.)

R. Cassiopeiæ. 1886, September 17th. 8·7 mag. A large ill-defined deep red star. 1887, February 16th. 9·5 mag. Very red, and not so sharply defined as the other stars in the field. March 13th. 8·7 mag. Very red. Cannot be focussed clear and sharp. September 19th. 7·6 mag. There is great difficulty in getting a good image. The star seems as if surrounded by a dense atmosphere. October 24th. 7·2 mag. With power 136 it seems surrounded by an atmosphere, or as if shining through fog. 1889, March 11th. 9·6 mag. Very hazy and ill-defined as if shining through ruddy mist. Other stars sharp. July 21st. 8·9 mag. Extremely deep ruddy. Ill-defined and hazy, quite unlike surrounding stars. ("a" night.)

S. Herculis. 1891, September 27th. 13 mag. A very minute point. October 14th. No star visible with powers 80 or 34. With 132 a faint nebulosity is suspected in the place of the variable; on slightly swaying the telescope a bluish nebulosity is certainly seen. October 28th. The nebulosity is best seen with power 136. The minute stars near the place of the variable are well seen, clear and sharp as usual. November 25th. The variable has reappeared as a minute but sharply-defined star.

[It is difficult to conceive of physical changes taking place rapidly on the vast scale indicated by Mr. Peek and Mr. Grover's observations. Possibly the blue nebulous haze or corona round these variable stars may only become visible to us when their light is small—as the variable star increases in brightness it seems possible that the light of the star diffused in our own atmosphere may blot out or eclipse the faint light of the nebula surrounding the variable.

During a total eclipse the light of the solar corona becomes visible through the illuminated atmospheric veil for the last few minutes before the "totality." If we could suppose our sun to be a variable star, and its light to be diminished till it equalled that of the thin crescent of photosphere left visible a few minutes before totality, we should see the sun under ordinary daylight conditions as surrounded by the corona, but as the light of the solar disc increased, the corona would be hidden by the solar light, diffused by our atmosphere, without any change necessarily taking place in the brightness of the corona.—A. C. Ranyard.]

THE CAUSE OF THE ICE AGE.

To the Editor of Knowledge.

Sir,—Since my letter on the cause of the Ice Age I have seen Sir R. Ball's book on the subject, in which he cites the passage (from 368*c* of the *Outlines*) on which he relies as establishing Herschel's inaccuracy. It is, no doubt, carelessly worded, and goes some way towards justifying the criticism, but I think the context shows that it was at worst a slip of the pen. Sir John Herschel first gives the usual explanation of the seasons, in which it is obviously implied that each hemisphere receives less heat during its winter than during its summer; then he proceeds (368*a*), "Let us now consider how these phenomena are *modified* by the ellipticity of the earth's orbit," &c.; and in considering the modification, he does not again refer to this unequal distribution of heat. In 368*c*, however, he says that at the period of greatest eccentricity if the perihelion took place at midwinter (for the northern hemisphere) "the state of the two hemispheres would be strongly contrasted. In the northern we should have a short but very mild winter with a long but very cool summer, *i.e.*, an approach to perpetual spring; while the southern hemisphere would be inconvenienced and might be rendered uninhabitable by

* Note.—The letter "a" means a night of excellent definition. Other stars with regard to which similar remarks appear in my note-book, S. Cassiopeiæ, R. Tauri, R. Aurigæ, V. Cancri, R. Ursæ Majoris, S. Ursæ Majoris, R. Camelopardi, R. Bootis, S. Coronæ, R. Aquilæ, S. Cephei.

the fierce extremes caused by concentrating *half* the annual supply of heat into a summer of very short duration, and spreading *the other half* over a long and dreary winter, &c." The language is, no doubt, incautious, but I think Sir John Herschel should be reasonably understood as speaking of half the supply of heat received, not by each hemisphere, but by the earth as a whole. Otherwise there would be no such contrast. Whether *called* winter or summer, each hemisphere would have a short hot season and a long cold one—each hemisphere receiving one-half of its heat in 166 days and the other half in 199. Moreover, the equable distribution—"the approach to perpetual spring"—would occur when the eccentricity was not at a maximum but at a minimum, and if the orbit became circular there would be no seasons at all. At all events, the erroneous statement forms no part of Herschel's general theory, which proceeds on the assumption that each hemisphere receives less heat during its winter than during its summer, owing to the obliquity of the ecliptic; and I cannot find that the inaccurate language which I have cited misled Croll.

10th February, 1892. W. H. S. MONCK.

A CURIOUSLY ERRONEOUS STATEMENT.

To the Editor of KNOWLEDGE.

SIR,—In your January number Sir Robert Ball attributes to Sir John Herschel a statement about the solar heat received by the earth, "to the effect that the heat was equally distributed, so that fifty parts were received in summer and fifty parts in winter." He says besides, that "Croll and other writers adopted Herschel's mistake as a scientific verity, and reared on this untrustworthy foundation no small superstructure of figures and arguments."

These grave charges against Sir John Herschel and Dr. Croll previously appeared, perhaps notwithstanding some friendly remonstrance, in a little book published by Sir Robert Ball under the title of *The Cause of an Ice Age*, where we are told (twice upon one page, 89) that this amazing assertion of equality has often been made; although the book itself (page 128) speaks of "the familiar difference between the mean temperature of summer and that of winter," adding that we may well be surprised "when we notice that the contrast in warmth between the two seasons is not a much larger one than our experience has shown it to be."

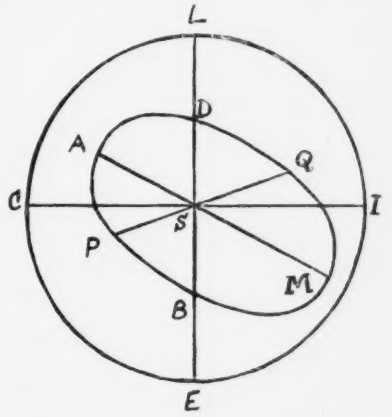
Yet Sir Robert Ball is so sure this mistake was actually made by Sir John Herschel, and often accepted by others, that its correction seems to be given (chapter vi.) as the principal reason why his book has been written.

In Herschel's *Outlines* (Articles 336, 337), a few pages before the passage cited by Sir Robert Ball in proof of his charge, that illustrious astronomer says (referring to a diagram in which A is the vernal equinox, B the summer solstice, C the autumnal equinox, and D the winter solstice), "Whenever, then, the sun remains more than twelve hours above the horizon of any place, and less beneath, the general temperature of that place will be above the average; when the reverse, below. As the earth, then, moves from A to B, the days growing longer, and the nights shorter, in the northern hemisphere, the temperature of every part of that hemisphere increases, and we pass from spring to summer; while at the same time the reverse obtains in the southern hemisphere. As the earth passes from B to C, the days and nights again approach to equality—the excess of temperature in the northern hemisphere above the mean state grows less, as well as its defect in the southern; and at the autumnal equinox C the mean state round again to A, all the same phenomena, it is obvious, must again occur, but reversed—it being now winter in the northern, and summer in the southern hemisphere. All this is exactly consonant to observed fact. The continual day within the polar circles in summer, and night in winter, the general increase of temperature and length of day as the sun approaches the elevated pole, and the reversal of the seasons in the northern and southern hemispheres, are all facts too well known to require further comment."

The law of the distribution of heat, which, according to Sir Robert Ball's book (p. 118), had escaped the attention of Herschel, is here stated, not with mathematical accuracy, because it was thought too well known to need more elucidation, but with quite unquestionable clearness.

The paragraph from Herschel's *Outlines*, quoted and corrected in *The Cause of an Ice Age*, does not appear in the earlier editions; it seems to have been printed for the first time in the fifth. In the fourth edition, published in 1851, Article 368 was as follows:—

"(368.) The elliptic form of the earth's orbit has but a very trifling share in producing the variation of temperature corresponding to the difference of seasons. This assertion may at first sight seem incompatible with what we know of the laws of the communication of heat from a luminary placed at a variable distance. Heat, like light, being equally dispersed from the sun in all directions, and being spread over the surface of a sphere continually enlarging as we recede from the centre, must, of course, diminish in intensity according to the inverse proportion of the surface of the sphere over which it is spread; that is, in the inverse proportion of the square of the distance. But we have seen (art. 350) that this is also the proportion in which the *angular velocity* of the earth about the sun varies. Hence it appears, that the *momentary supply of heat* received by the earth from the sun varies in the exact proportion of the angular velocity, *i.e.* of the *momentary increase of longitude:* and from this it follows, that equal amounts of heat are received from the sun in passing over equal angles round it, in whatever part of the ellipse those angles may be situated. Let, then, S represent the sun; A Q M P the earth's orbit; A its nearest point to the sun, or, as it is called, the *perihelion* of its orbit; M the farthest, or the *aphelion;* and therefore A S M the *axis* of the ellipse. Now, suppose the orbit divided into two segments by a straight line P S Q, drawn through the sun, and anyhow situated as to direction; then, if we suppose the earth to circulate in the direction P A Q M P, it will have passed over 180° of longitude in moving from P to Q, and as many in moving from Q to P. It appears, therefore, from what has been shown, that the supplies of heat received from the sun will be equal in the two segments, in whatever direction the line P S Q be drawn. They will, indeed, be described in unequal times; that in which the perihelion A lies in a shorter, and the



other in a longer, in proportion to their unequal area: but the greater proximity of the sun in the smaller segment compensates exactly for its more rapid description, and thus an equilibrium of heat is, as it were, maintained. Were it not for this, the excentricity of the orbit would materially influence the transition of seasons. The fluctuation of distance amounts to nearly $\frac{1}{30}$th of its mean quantity, and, consequently, the fluctuation in the sun's direct heating power to double this, or $\frac{1}{15}$th of the whole. Now, the perihelion of the orbit is situated nearly at the place of the northern winter solstice; so that, were it not for the compensation we have just described, the effect would be to exaggerate the difference of summer and winter in the southern hemisphere, and to moderate it in the northern; thus producing a more violent alternation of climate in the one hemisphere, and an approach to perpetual spring in the other. As it is, however, no such inequality subsists, but an equal and impartial distribution of heat and light is accorded to both."

In the fifth edition, published in 1858, this Article was replaced by the three following:—

"(368 *a*.) Let us next consider how these phænomena are modified by the ellipticity of the earth's orbit and the position of its longer axis with respect to the line of the solstices. This ellipticity (art. 350) is about one sixtieth of the mean distance, so that the sun, at its greatest proximity is about one thirtieth of its mean distance nearer us than when most remote. Since light and heat are equally dispersed from the sun in all directions, and are spread, in diverging, over the surface of a sphere enlarging as they recede from the center, they must diminish in intensity according to the inverse proportion of the surfaces over which they are spread, *i.e.* in the inverse ratio of the squares of the distances. Hence the hemisphere opposed to the sun will receive in a given time, when nearest, two thirtieths or one fifteenth more heat and light than when most remote, as may be shown by an easy calculation. Now, the sun's longitude when at its least distance from the earth (at which time it is said to be in perigee and the earth in its perihelion) is at present 280° 28′ in which position it is on the 1st of January, or eleven days after the time of the winter solstice of the northern hemisphere; or, which is the same thing, the summer solstice of the southern (art. 364), while on the other hand the sun is most remote (in apogee or the earth in its aphelion), when in longitude 100° 28′ or on the 2nd of July, *i.e.* eleven days after the epoch of the northern summer or southern winter solstice. We shall suppose, however, for simplicity of explanation, the perigee and apogee to be coincident with the solstice. At and about the southern summer solstice then, the whole earth is receiving *per diem* the greatest amount of heat that it can receive, and of this the southern hemisphere receives the larger share, because its pole and the whole region within the antarctic circle is in perpetual sunshine, while the corresponding northern regions lie in shadow. On the other hand, at and about the northern summer solstice, although it is true that the reverse conditions as to the regions illuminated prevail, yet the whole earth is then receiving *per diem* less heat owing to the sun's remoteness: so that on the whole *if the seasons were of equal duration*, or in other words, if the angular movement of the earth in its elliptic orbit were uniform, the southern hemisphere would receive more heat *per annum* than the northern, and would consequently have a warmer mean temperature.

"(368 *b*.) Such, however, is not the case. The *angular* velocity of the earth in its orbit, as we have seen (art. 350), is not uniform, but varies in the inverse ratio of the square of the sun's distance, that is, in the same precise ratio as his heating power. The momentary supply of heat then received by the earth in every point of its orbit varies exactly as the momentary increase of its longitude, from which it obviously follows, that equal amounts of heat are received from the sun in passing over equal angles round it, in whatever part of the ellipse those angles may be situated. Supposing the orbit, then, to be divided into two segments by any straight line drawn through the sun, since equal angles in longitude (180°) are described on either side of this line, the amount of heat received will be equal. In passing then from either equinox to the other, the whole earth receives equal amount of heat, the inequality in the intensities of solar radiation in the two intervals being precisely compensated by the opposite inequality in the duration of the intervals themselves; which amounts to about $7\frac{1}{2}$ days, by which the northern spring and summer are together longer than the southern. For these intervals are to each other in the proportion of the two unequal segments of the whole ellipse into which the line of the equinoxes divides it (see art. 353).

"(368 *c*.) In what regards the comfort of a climate and the character of its vegetation, the intensity of a summer is more naturally estimated by the temperature of its hottest day, and that of a winter by its sharpest frosts, than by the mere durations of those seasons and their total amount of heat. Supposing the excentricity of the earth's orbit were very much greater than it actually is; the position of its perihelion remaining the same; it is evident that the characters of the seasons in the two hemispheres would be strongly contrasted. In the northern, we should have a short but very mild winter, with a long but very cool summer—*i.e.* an approach to perpetual spring; while the southern hemisphere would be inconvenienced and might be rendered uninhabitable by the fierce extremes caused by concentrating half the annual supply of heat into a summer of very short duration and spreading the other half over a long and dreary winter, sharpened to an intolerable intensity of frost when at its climax by the much greater remoteness of the sun."

Now, it is quite clear that Sir Robert Ball's suggestion attributing the origin of the mis-statement to Sir J. Herschel's having hastily confused two very distinct things, can by no means have been correct; because the remarkable proposition mentioned on page 120 of *The Cause of an Ice Age* was stated very clearly in the paragraph of the *Outlines* next preceding that which Sir R. Ball incriminates; and there Sir John Herschel was dealing with actual fact, whereas in the subsequent paragraph he was considering a supposititious case.

Surely it is more reasonable to suppose that he used the word *half* (with extraordinary lack of his usual carefulness), instead of *part*, to import mere division, without regard to quantity. This use of the word cannot possibly be defended, but the most careful writers are liable to similar slips; and it seems inconceivable that he could have meant to represent the heat received by either hemisphere during the year as equally divided between summer and winter, under any circumstances whatever. He includes heat and light as subject to the same law of diffusion, and who could possibly think or say that the summer and winter light could ever be equal?

We need not look far for inaccuracies somewhat analogous. The quotation from Sir Robert Ball, first above cited, reads as if he accused Herschel of formally teaching the alleged equality to be the normal or constant state of matters, but his indictment can hardly be meant to go so far as that. His book speaks of the mistake as an inadvertence (page 135).

Again, on page 27 of *The Cause of an Ice Age*, he says, "We have learned that one hemisphere was once covered

with ice," but this cannot mean, although it appears to say, that the ice-cap ever reached the equator in either hemisphere. Suppose that someone, reading it literally, were to maintain this to be the author's belief, would not Sir Robert Ball be entitled to call the assertion monstrous? Two pages before, he had spoken of "the sunny regions which seem never to have been invaded by the desolation of an Ice Age"; afterwards he twice mentions a huge sheet of ice over "a large part of" one or the other hemisphere; and, in the concluding chapter, speaks of the great ice-sheet as invading central Europe as far as Saxony, covering the greater part of Great Britain, submerging Canada, and burying much of that tract which now forms the Eastern States of North America. All these passages are just as incompatible with belief in an ice-cap extending to the equator, as the whole tenor of Sir John Herschel's teaching is with alleging the equality of summer and winter mean temperature.

There was once some difference between the teaching of Sir John Herschel and that of Sir Robert Ball about the astronomical theory of the Ice Age, but comparison of the articles above quoted from the fourth and fifth editions of Herschel's *Outlines* will show that the author deliberately withdrew his earlier contention that the eccentricity of the earth's orbit did not materially affect the transition of seasons. The statement in the later editions seems to anticipate Dr. Croll's and Sir Robert Ball's theory, that ice-ages can happen only when, the eccentricity being at or near its maximum, the line of equinoxes is perpendicular to the major axis of the earth's orbit, or nearly so.

Strangely enough, Dr. Croll, who is supposed to have submissively adopted Herschel's teaching, appears to have been unaware of this change, although it was made seventeen years before the publication of *Climate and Time*; for in his introduction to that book, and also in its first appendix, he quotes and criticises article 368, as it appeared in the earlier editions of the *Outlines*.

It is necessary to mention only two passages in *Climate and Time*, in order to prove that Dr. Croll was not one of those who have asserted the heat received from the sun during summer to be equal to that received during winter. On page 55 he says that change in the eccentricity of the earth's orbit may affect climate by increasing or diminishing the difference between summer and winter temperature; thus showing his knowledge of the familiar fact that some difference does, and always must, exist. And on page 87 he twice uses the relation of 7 to 4 in dealing with this difference; applying it in the first instance to the proportion between summer and winter sunshine in the latitude of Edinburgh, and in the second to the proportion between winter and summer nights in either hemisphere. Is not the ratio of 7 to 4 sufficiently near that of Sir Robert Ball's 63 to 37, for the purpose of protecting Dr. Croll from the imputation of having carelessly adopted the error, inadvertently suggested by a statement which there is some reason for believing that he had never seen?

Your obedient servant,

Newcastle upon Tyne, B. Noble.
17th February, 1892.

CAMPHIRE AND CAMPHOR.

By J. Ch. Sawer, F.L.S.

IN the 14th verse of the 1st chapter of the Song of Solomon we find: "My beloved is unto me as a cluster of Camphire in the vineyards of Engedi." At first sight the meaning of this verse is obscure. The word translated "Camphire" is certainly not intended to convey to the mind any idea of the substance we now know as Camphor; Solomon was very happy in his choice of similes, and such a comparison would have been absurd. In the original of this poem or love-song, written about 1000 years B.C., the word translated Camphire is *Cophérim*, the Egyptian equivalent of which is *Hennah*. In Egypt, on one of the nights before a wedding, "Hennah" is applied with linen bandages to the hands and feet of the bride until the next morning, when they appear of that rosy red which Egyptians believe to be love's proper hue. This night, in the order of the marriage ceremonials, is called "the night of the Hennah." The word "cluster," found in the text, no doubt refers to the flowers of this plant, which are of a golden yellow and are borne in clusters; they are remarkably fragrant whether fresh or dry, and were much esteemed by women in the East, especially the Jewish women, who carried bouquets of them in their bosoms and twined them into crowns for their heads.

This Hennah is Pliny's "Cyprus of Egypt," and the women of Egypt and other Eastern countries stain not only the palms of their hands and the soles of their feet with a paste of Hennah leaves, but also the tips of the fingers, the nails and the knuckles, from which custom probably arises the designation of Aurora as "rosy," or "rosy-fingered" (ῥοδοδάκτυλος ἠώς).

A considerable business is done in the leaves of the Hennah, which are collected in the green state and dried in the sun. The leaves are then coarsely powdered and beaten up with Catechu, and the freshly made paste is laid on at bed-time and renewed in the morning; it leaves a peculiar reddish-orange stain, ruddy, and somewhat similar to the colour of red ochre, though hardly so deep, which lasts on the skin and nails for some three or four weeks, until removed by renewed growth. This colouration is much admired by Mussulmans in India and many Eastern countries.

On moistening the dried leaves a slight odour is perceptible; their taste is bitter and faintly aromatic, owing to an essential oil and tannin contained in them. A medicament prepared from them is employed by the Arabs in the treatment of wounds of all kinds, causing the skin to grow over and healing them very quickly; these effects are probably due to some exciting action of the essential oil and the astringent properties of the tannin. They use this remedy principally on horses, to heal wounds or sores caused by friction of the harness or otherwise on a journey. Having stopped for a rest and unsaddled, they will apply a plaister to it and continue their march without more thought of the wound, which, if it does not heal, at least does not extend by friction, and causes less suffering to the horse. They also employ it as a means of preventing the opening of old wounds where the hair has not grown over the scar. It closes and hardens the tissues—in fact, tans the skin. For similar reasons, the Arabs who can afford to indulge in the sport of gazelle hunting will give their horses a foot-bath of Hennah, especially if the animals are young, or have not taken exercise for some time. There may be some analogy to this in the custom prevailing amongst Arabian women of staining the palms of their hands and the soles of their feet—it may render the skin less tender.

This shrub, being known to the Arabs as "Henné-al-hennah," appears to be a native of Arabia. It has been cultivated from its earliest times, and is now very common throughout India, Cabul, and Persia, as well as along the coast of the Mediterranean. Botanically, it is now known to us as the *Lawsonia inermis* of Linnæus. It flowers and seeds most of the year, and is much used for hedges,

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growing readily from cuttings. It has been introduced into the West Indies, and is there known as Jamaica Mignonette. The perfume of the flowers is rather that of a mixture of rose and mignonette. Acids destroy the dye yielded by the leaves, but alkalies and infusions of astringent plants deepen it; although this juice stains the epidermis, it does not communicate any colour to cloth.

The name of the white crystalline substance commonly known as Camphor is derived from the Arabic word "Káfûr," which in its turn was derived from the Sanskrit word "Kapûra," signifying *white*, or a pure substance.

The old English name for Camphor was spelt *Camphire*, and as the translators of the Bible may have been unacquainted with the botany and natural history of the plant Hennah, they may have confounded the Hebrew word "Cophérim," or "Kopher," with the Arabic word "Káfûr." This is pure surmise, but evidently the word "Hennah" should be substituted for the word "Camphire" in the Song of Solomon. Moreover, there is no record of the substance Káfûr, or Kápûra, being known in Solomon's day.

Camphor is first mentioned by Arabian writers in the sixth century. It is mentioned by Aëtios, of Amida, in Mesopotamia, according to whom Kaphura is a rare and wonderful medicine. It is again mentioned, together with musk, amber, and santal wood, among the treasures taken in the year 636, by the Kalif Omar, at the plundering of the Sassanides Palace, in Madain, on the Tigris, and is subsequently noticed as a costly gift, often presented by Indian princes to high Chinese officials. This Camphor came from the land known as Kaïsûr, the present Sumatra.

Ishak Ibn Amrân, an Arabian physician, who lived towards the close of the ninth century, states that the best Camphor was produced in Fansûr, a locality which was visited by Marco Polo in the thirteenth century, who mentions that the Kaïsûr Camphor was then marketable at its weight in gold.

From various sources of information it may be concluded that Camphor, as it was first known, was the variety which exists ready formed in the pith cavities of the trunk of the *Dryobanalops camphora*, a magnificent tree growing in Borneo, Sumatra, and Labua. In the forests of Sumatra these trees attain an immense size, often being found of 6 or 7 feet in diameter. They do not all contain Camphor, many of them containing an oil, which is supposed to be the first stage of the formation of the drug, and would develop into Camphor were the tree left unmolested. Both oil and Camphor are found in the heart of the tree, not occupying the whole length of the pith cavity, but often in spaces of a foot or a foot and a half in length, at intervals. The method of extracting the oil is merely by making a deep incision with a Malay axe, about 14 or 18 feet from the ground, till near the heart, when a narrower incision is made, and the oil, if any in the tree, gushes out, and is received in bamboos or other utensils. In this manner a party proceeds through the woods, wounding the Camphor trees till they attain their object. From a tree containing both oil and Camphor, 2 gallons of the former and 3 lbs. of the latter may sometimes be obtained, but hundreds of trees may be mutilated before Camphor is discovered, as the natives have no certain means of ascertaining if the tree produces either the one or the other. When Camphor is found, the tree is felled and cut into junks of a few feet long; these are then split, and the Camphor is found in the heart, occupying a space in circumference of the thickness of a man's arm. The quantity varies from 3 to 15 lbs., and rarely as much as 20 lbs. are obtained. Some trees when felled are not found to contain any at all. The Camphor thus found is called *Tentary*, and by reason of the small quantity, it commands a high price—from 35 to 70 shillings a pound, according to quality. It does not find a market in Europe at all, but is used to some extent on the spot as incense, especially in the observation of funeral rites and embalming the bodies of the dead, and is exported to China, Japan, and other places in Eastern Asia, for similar purposes. It is heavier than Laurel Camphor, and sinks in water.

The date at which the Chinese discovered the production of Camphor from the *Laurus camphora* is unknown. This is called Laurel Camphor, or "Common" Camphor. It was brought into Europe by the Arabians about the twelfth century, which is proved by the mention made of it by the Abbess Hildegard ("St. Hildegardis Opera omnia," 1145, published in Paris 1855) who called it Ganphora. Garcia de Orta, writing in 1563, states that the Chinese is the only Camphor imported into Europe; that of Borneo and Sumatra, being a hundred times more valuable, is retained by the Orientals for their own use. Kämpfer (strange coincidence of names), who visited Japan 1690–92 and made a drawing of the Japanese Camphor tree under the name of *Laurus camphorifera*, expressly declares that the tree differs entirely from the Camphor-yielding tree of the Malay Archipelago. He further states that the Borneo Camphor is one of the most precious articles of merchandise imported into Holland from Japan. This Camphor was refined in Japan by a process long kept secret.

The common Camphor tree, *Laurus camphora*, is distributed throughout the eastern provinces of Central China, on the Island of Hainan, and very extensively in Formosa. It also occurs as a forest-tree on the islands Kiushiu and Shikoku of South Japan, its growth being much more vigorous there than in the more northern districts. The Camphor of European commerce is produced almost exclusively from the Camphor laurels of Formosa and Japan.

The large and increasing quantities of this drug consumed in all civilized countries make the question of its continued production and regular supply a matter of considerable importance. It is a well-known fact that the distillation of the crude Camphor from the wood is conducted in a primitive careless way, which causes great waste. The Camphor laurels of Formosa are gradually being destroyed under the careless system employed by the Chinese gatherers. In fact they have been entirely exterminated along the sea-board, and the wood is now obtained in the forests along the frontier, between the settlements of the Chinese and the inland mountainous regions still occupied by the aboriginal population. The Camphor-gatherers are hence continually exposed to the assaults of the natives, which interrupt the profitable prosecution of this industry. No attempts are made to cultivate laurels to take the place of those destroyed, and a sufficient quantity of the drug is only obtained by constant encroachments upon the territory of the Formosans, destroying the trees still further into the interior at every new move.

The method of extracting the Camphor is as follows:—The trees are felled and the small branches chopped up; these, with the chips and twigs, are alone used, the heavy wood being abandoned. A long trough, made of a hollow tree, and coated with clay, is placed over eight or ten hearth fires, and is half filled with water. Boards, perforated with holes, are put across the trough, and above each hole is a jar filled with chips of the wood, with

earthenware pots inverted above them, the joints being made tight by hemp and clay. The water in the trough is heated to boiling, and the steam passing through the holes saturates the chips, causing the Camphor to sublime and condense in crystals in the inverted pots above. The Camphor thus obtained is sent into the interior of the island, to Tasmin, the principal port, packed in baskets covered with cloths and large leaves. On arrival, it is re-packed in tubs or lead-lined cases for export by Chinese vessels to Hong Kong, Shanghai or Canton, the loss by evaporation while in transit from the place of its production being very large. A yellow oil exudes from the packages of this crude Camphor, locally known as "Oil of Camphor," and is used medicinally. The Formosa Camphor, which sometimes goes by the name of "Chinese Camphor," occasionally arrives in India in a semi-fluid state, owing to the addition of water before shipment.

The Japan Camphor used to be extracted, according to Kämpfer (the authority above referred to), by boiling the wood with water in an iron kettle, and condensing the vapour in an earthenware dome, closed at the top with rice-straw. The modern practice is to distil the wood with water in an iron retort fitted with a wooden dome, from which the vapours are led through a bamboo tube to the cooling apparatus. This consists of a wooden box, containing seven transverse compartments, and is enclosed in a second box through which water is allowed to flow; the vapours are conducted through all the compartments in succession by means of holes placed alternately at either end of the dividing walls. The Japan Camphor arrives dry; it is lighter in colour, and somewhat pinkish. It arrives in double tubs (one within the other) without metal lining; hence it is sometimes called "tub-Camphor."

The European process of refining Camphor was long kept a secret, and towards the end of the seventeenth century the entire Camphor of Europe had to be sent to Holland to be sublimed. A monopoly was also held for some time in Venice, but at the present day Camphor refining is largely accomplished in England, Holland, Hamburg, Paris, New York and Philadelphia. Before describing the correct European method, it may be well to describe the fraudulent method adopted in India, the artful peculiarity of which is to get as much interstitial water as possible into the Camphor cake. The vessel used is a tinned cylindrical copper drum, one end of which is removable; into this is put 14 parts of crude Camphor, and $2\frac{1}{2}$ parts of water; the cover is then luted with clay, and the drum being placed upon a small furnace made of clay is also luted to the top of the furnace. In Bombay, four of such furnaces are built together, so that the tops form a square platform. The sublimation is completed in about three hours; during the process, the drums are constantly irrigated with cold water. Camphor sublimed in this way is not stored, but distributed at once to the store-keepers before it has had time to lose weight by drying. It is sold at the same price as the crude article, the refiner's profit being derived from the introduction of water. The same practice seems to be followed at Delhi, and at a few other cities in India.

In Europe, it is usually refined by mixing it with lime, charcoal, or iron filings, and subliming the mixture in large glass vessels; cakes weighing eight to twelve pounds being thus obtained.

The process adopted in Philadelphia is devised in such a way as to obtain the sublimate in the form of a finely powdered snowy mass, to accomplish which, about one-tenth per cent. of water is added to the crude material. The apparatus consists of a flat iron chamber, capable of holding about 200 lbs., connected by means of an iron tube with a condensing chamber eight feet long, four feet wide, and four feet high. This chamber is constructed of enamelled bricks set in Portland cement, forming an arched roof and floor of the same material. After an operation the apparatus is allowed to remain undisturbed over night, to become sufficiently cool. On removal of the sublimate, it is compressed into moulds by hydraulic pressure of 2500 lbs. to the square inch, and the finished product obtained in small cakes, highly compressed, and weighing one ounce.

Camphor forms a tough crystalline mass of characteristic taste and odour, and can only be powdered when it is moistened with alcohol or some other solvent. It dissolves in 1300 parts of water at 20° C., and at 12° C. in 0·8 parts of alcohol of sp. gr. 0·806. It is readily soluble in ether, acetone, chloroform, benzine, and other hydro-carbons; also in glacial acetic acid and in carbon disulphide. It melts at 175° C., and boils at 204° C., but volatizes very rapidly at the ordinary temperature and sublimes, when kept in close vessels, in lustrous hexagonal crystals which frequently form splendid stars.

Camphor oil is used for mixing with fine lac varnishes, rendering them less liable to crack. It is a powerful antiseptic and disinfectant, and covers the smell of mineral oils.

THE FACE OF THE SKY FOR MARCH.

By Herbert Sadler, F.R.A.S.

SUN-SPOTS are rapidly increasing in number and size, a group being distinctly visible to the naked eye at the time of writing these lines. The following are conveniently observable minima of some Algol-type variables (*cf.* "Face of the Sky," for February). Algol.—March 10th, nine minutes after midnight; March 13th, 8h. 58m. P.M. S Cancri.—March 16th, 11h. 46m. P.M. The student will, of course, keep a watch on Nova Aurigæ, at present about the fifth magnitude, the place of which for 1892 is 5h. 25m. 3s. +30° 21′. Its spectrum seems to resemble in some particulars those shown by Novæ 1866 and 1876.

Mercury is in superior conjunction with the Sun on the 6th, and after that he rapidly improves in position, setting on the 16th at 7h. 1m. P.M., 56m. after the Sun, with an apparent diameter of $5\frac{1}{2}''$, and a northern declination of 2° 15′, $\frac{92}{100}$ths of the disc being illuminated. On the 21st he sets at 7h. 38m. P.M., or 1h. 24m. after the Sun, with an apparent diameter of 6″, and a northern declination of 6° 53′, $\frac{80}{100}$ths of the disc being illuminated. On the 26th he sets at 8h. 9m. P.M., or 1h. 47m. after sunset, with an apparent diameter of $6\frac{1}{2}''$, and a northern declination of 10° 56′, $\frac{61}{100}$ths of the disc being illuminated. On the 31st, when he is at his greatest eastern elongation (18° 54′), he sets at 8h. 28m. P.M., or just upon two hours after the Sun, with an apparent diameter of $7\frac{1}{2}''$, and a northern declination of 13° 58′. The student may be reminded that Mercury is at his greatest brilliancy about ten days or a fortnight before his greatest eastern elongation, notwithstanding that at his greatest eastern elongation he sets considerably later in the evening. While visible, Mercury describes a direct path through Pisces to the borders of Aries.

Venus is now becoming a very bright object in the evening sky. She sets on the 1st at 9h. 18m. P.M., with a northern declination of 7° 52′, and an apparent diameter of $14\frac{1}{2}''$, $\frac{76}{100}$ths of the disc being illuminated. On the 21st she sets at 10h. 18m. P.M., with a northern declination of 17° 8′, and an apparent diameter of $16\frac{1}{2}''$, $\frac{69}{100}$ths of the

disc being illuminated, and her brightness being about one-half of what it will be at the beginning of June. On the 31st she sets at 10h. 48m. P.M., with a northern declination of 20° 53′, and an apparent diameter of 17½″, $\frac{65}{100}$ths of the disc being illuminated. In the early evening of the 28th an 8½ magnitude star will be situated very near the planet. During the month she passes from Pisces, through Aries, into Taurus. Mars is invisible, and Jupiter is in conjunction with the Sun on the 21st.

Saturn is well-placed for observation, being in opposition to the Sun on the 16th, at a distance from the earth of about 787¼ million miles. He rises on the 1st at 6h. 55m. P.M., with a northern declination of 3° 5′, and an apparent equatorial diameter of 19·2″ (the major axis of the ring system being 44·1″ in diameter, and the minor 1·9″). On the 31st he rises at 4h. 45m. P.M., with a northern declination of 4° 2′, and an apparent equatorial diameter of 19·2″ (the major axis of the ring system being 44·1″ in diameter, and the minor 1·1″). The following phenomena of the satellites may be observed (the times are given to the nearest quarter of an hour). March 3rd, 8¼h. P.M., Rhea, eclipse disappearance. March 4th, 3¾h. A.M., Titan, eclipse disappearance. March 5th, 0¾h. A.M., Dione, eclipse disappearance. March 6th, 4¼h. A.M., Tethys, eclipse disappearance. March 9th, 1¼h. A.M., Tethys, eclipse disappearance; 6 P.M., Iapetus, at greatest W. elongation. March 10th, 11h. P.M., Tethys, eclipse disappearance. March 11th, 10½h. P.M., shadow of Titan in central transit. March 12th, 8¼h. P.M., Tethys, eclipse disappearance; 9h. P.M., Rhea, eclipse disappearance. March 18th, 8½h. P.M., Dione, eclipse reappearance. March 22nd, 1½h. A.M., Rhea, eclipse reappearance. March 26th, 4¼h. A.M., Tethys, eclipse reappearance. March 27th, 1½h. A.M., Dione, eclipse reappearance; 9¾h. P.M., shadow of Titan in central transit. March 28th, 1¼h. A.M., Tethys, eclipse reappearance. March 29th, 7h. P.M., Dione, eclipse reappearance; 10¾h. P.M., Tethys, eclipse reappearance. March 31st, 2½h. A.M., Rhea, eclipse reappearance; 8¼h. P.M., Tethys, eclipse reappearance. At 10h. P.M. on the 17th, a 9th magnitude star will be about 1½′ north of the planet. During March Saturn describes a short retrograde path in Virgo, without approaching any naked-eye star.

Uranus is coming into a good position for observation, rising on the 1st at 10h. 37m. P.M., with a southern declination of 13° 0′, and an apparent diameter of 3·7″. On the 31st he rises at 8h. 34m. P.M., with a southern declination of 12° 43′. He will be so favourably placed for observation during March, never being more than 30′ from the 4½ magnitude star λ Virginis, that the student should endeavour to pick the planet up with the naked eye or an opera glass. His occultation by the Moon is mentioned below. A map of the path of Uranus is given in the *English Mechanic* for February 12th. Neptune is still visible, rising on the 1st at 9h. 43m. A.M., with a northern declination of 19° 50′, and an apparent diameter of 2½″. On the 31st he sets at 11h. 36m. P.M., with a northern declination of 19° 56′. He describes a short direct path to the N.W. of ε Tauri.

There are no very well-marked showers of shooting stars in March. The zodiacal light should be looked for over the western horizon on every moonless evening.

The Moon enters her first quarter at 7h. 15m. P.M. on the 5th; is full at 0h. 55m. P.M. on the 13th; enters her last quarter at 5h. 16m. P.M. on the 21st; and is new at 1h. 18m. P.M. on the 28th. She is in apogee at 9·7h. P.M. on the 15th (distance from the earth 252,300 miles); and in perigee at 10h. P.M. on the 28th (distance from the earth 221,960 miles). The greatest western libration is at 6h. 10m. A.M. on the 7th, and the greatest eastern at 3h. 14m. A.M. on the 23rd. The planet Uranus, equal to a 5½ magnitude star, will be occulted at 0h. 30m. A.M. on the 17th, at an angle of 112°, reckoned as in double star observation, *i.e.*, from the true N. point in the direction N.E.S.W., and will reappear at 1h. 46m. A.M., at an angle of 318°.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

The solution of the February problem is withheld for the same reason as in the case of the previous problem.

Correct Solutions have been received from Giuoco Pianissimo, W. T. Hurley, and A. Rutherford. Of the MS. Problem, solutions in three moves from W. T. Hurley, A. Rutherford, and M. B. (Jesmond): the latter not quite complete.

R. G. Haig (*Melbourne*).—In the variation you mention the Queen mates at QRsq. This is probably the leading variation, and no doubt you will have seen the mate before this.

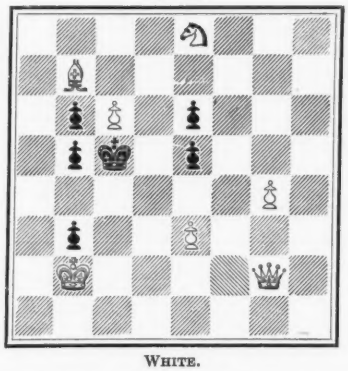
Alpha.—The move you rejected is, strange to say, the correct key-move, though rather strong-looking. In reply to the move you suggest, Black must not play 1. . . P × Kt, or 1. . . . P to B3; but he may play apparently almost anything else. Certainly there is not much pleasure in finding more than one solution to any problem.

M. B. (*Jesmond*).—In the three-move problem you omitted the important defences, Q to Kt6 (compelling Q to Kt4ch) and P to K5. Also the defence R to Kt3, in which a slight variation in the mating method occurs. After 1. . . . K × P, Kt to B5 is a *triple* continuation. In position A there is a dual on the third move—after 1. . . P becomes a Kt, etc.

PROBLEM.

[From *England*.—Composer unknown.]

Black.



White.

White to play, and mate in three moves.

"KNOWLEDGE" SOLUTION TOURNEY.

This contest has at length been decided. It will be seen from the list of correct solutions that, of the four surviving competitors, two only proved equal to the double task set last month. These two were Mr. A.

Rutherford, of Liverpool, and Mr. W. T. Hurley, of Rochester. Giuoco Pianissimo, besides solving the Four-move problem, found two solutions in four moves to the MS. problem. But as the problem was solvable in three moves, these solutions could not count. He was thus, strange to say, defeated by his own suggestion, that the number of moves should not be stated. This paradoxical result occurred also in the case of Mr. Hurley. The scoring of duals was his own suggestion and the sole cause of his defeat, as the following score will show :—

Solutions—A. Rutherford, 16 ; W. T. Hurley, 16.

Duals—A. Rutherford, 12; W. T. Hurley, 5.

Other Variations—A. Rutherford, 3 ; W. T. Hurley, 4.

Mr. Rutherford, of the Liverpool Chess Club, thus comes out the winner of the Knowledge Prize, and considering the quality of the opposition, must be congratulated on a really fine performance. Messrs. Hurley, Giu. Pianissimo and M. B. (Jesmond), also deserve congratulation, not unmingled with condolence, on the excellent fight they made. This award remains open for one week.

CHESS INTELLIGENCE.

The championship match at Havana will, perhaps, be over before the appearance of this number. At the time of writing the score is—Steinitz 7, Tschigorin 8, Drawn 4. Mr. Steinitz has been singularly unsuccessful against the Evans Gambit and the Two Knights Defence. Most of his victories have been obtained, as usual, in the Ruy Lopez and the Close Game. It is well known that Mr. Steinitz always reserves his finest play for the latter stages of an important contest, and it will therefore cause some surprise if he loses the match.

The contest for the championship of the City of London Club resulted in a tie between Messrs. Moriau, Mocatta and Dr. Smith, and is now reduced to a duel between the first and last named.

The British Chess Club Handicap is still undecided, pending the decision of a committee appointed to adjudicate on several unplayed games.

Game played by telephone on December 12th, 1891 :—

[Zukertort Opening.]

White (Liverpool). (Rev. J. Owen, Messrs. Cairns, Howard, and Kaizer.)	Black (British Chess Club). (Messrs. Donisthorpe, Hoffer, G. Newnes, M.P., and Trenchard.)
1. Kt to KB3	1. P to Q4
2. P to Q4	2. P to K3
3. P to K3	3. P to QB4 (*a*)
4. P to QKt3	4. Kt to KB3
5. B to QKt2	5. Kt to QB3
6. B to Q3	6. P to QKt3 (*b*)
7. Castles	7. B to Q3 (*c*)
8. QKt to Q2	8. Castles
9. P to QR3 (*d*)	9. B to Kt2
10. Q to K2	10. R to Bsq (*e*)
11. Kt to K5	11. Q to B2
12. P to KB4	12. P×P (*f*)
13. P×P	13. Kt to K2
14. QR to QBsq (*g*)	14. Kt to Kt3
15. P to KKt4	15. KR to Ksq (*h*)
16. P to KKt5	16. Kt to Q2
17. Q to KR5	17. Kt (Q2) to Bsq
18. R to KB3	18. B×Kt (*i*)
19. KBP×B	19. R to K2
20. QR to KBsq	20. P to QKt4 (*j*)
21. QR to KB2	21. P to R4
22. Kt to KBsq	22. B to B3
23. Kt to K3	23. B to Ksq
24. Kt to Kt4	24. P to KB4 (*k*)
25. KtP×P (*en passant*)	25. R to B2
26. R to R3	26. P×P (*l*)
27. Kt×P(ch) (*m*)	27. K to Rsq
28. Q to KR6 (*n*)	28. R to Kt2
29. R to Kt2	29. P to QKt5
30. P to QR4 (*o*)	30. Q to K2
31. B to QBsq	31. Q to R2 (*p*)
32. B to K3	32. B to B2
33. K to Rsq	33. B to Ksq
34. Kt×B	34. R×Kt
35. B×Kt	35. Resigns (*q*).

Notes.

(*a*) Not advisable so early for the second player, if indeed for either side. The proper line of development is by P to QKt3 and B to Kt2.

(*b*) P to QR3 followed by P to QKt4 would be useless, as White could stop any further advances by P to QB4.

(*c*) B to Kt2 seems preferable. The Bishop is bound to occupy that square sooner or later, while the proper square for the King's Bishop cannot yet be determined.

(*d*) To prevent Kt to QKt5, since White intend on their next move to block up their Bishop's retreat. They might, however, play Kt to K5 at once.

(*e*) They should have prevented Kt to K5 by Q to B2.

(*f*) Making matters worse by relieving White of their weak King's Pawn, and at the same time allowing the White Queen's Bishop to come into play ultimately on the King's side. The open QB file is inadequate compensation, Perhaps Kt to K2, with a view to Kt to K5, was better.

(*g*) To prevent Kt to K5 ; for after the exchange of pieces Black would otherwise win the QBP. White now obtain an irresistible attack, the opposing forces being completely shut in.

(*h*) To make room for one of the Knights at Bsq in view of the coming onslaught.

(*i*) Black's best chance lay in 18 . . . Q to K2; if then 19. P to QR4, P to KB4 ; 20. P×P *en passant*, Q×P, with a fairly defensible game.

(*j*) In order to be able to reply to B to QR3 subsequently by P to Kt5.

(*k*) The only way to prevent the mate threatened by Kt to B6ch, followed, if the Knight be taken, by P×Kt and Q to R6 ; or, if the King move, by Kt×RP and R to R3.

(*l*) White threatened to win at once by B×Kt.

(*m*) Kt to R6ch was of course good enough, but this is even stronger.

(*n*) We fail to see the objection to 28. B×Kt, Kt×B? 29. Q×Kt. If Black then pins the Queen by R to Kt2, mate follows in two moves. Again, if 28 . . . R to Kt2 ! ; 29. Kt×B, R×Kt; 30. R×Ktch, R×R ; 31. R to Kt3, QR to KKtsq ; 32. B to Q3, R×Rch ; 33. P×R, Q to KKt2 ; 34. K to Rsq !, followed soon by B to QBsq, and B to KKt5. In this variation, if Black play 33. . . . R×Pch, White must reply 34. K to B2 ; not 34. K to R2 ?, R×B ! and draws.

(*o*) If 30. Kt×B, R×Kt; 31. B×Kt, Kt×B; 32. R×Kt, R×R ; Q×R, R to Ktsq, and wins.

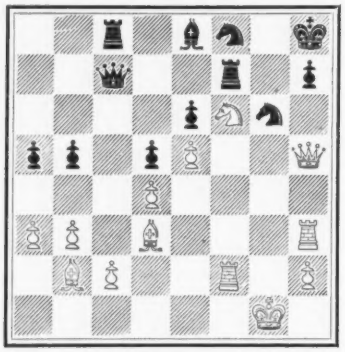
(*p*) If the Queen stays where she is, the Bishop goes to Kt5.

XUM

(*q*) Apart, perhaps, from their 28th move, the Liverpool players conducted their attack in the finest possible style. Black, perhaps, missed their chance at the 18th move, but afterwards could not have done more than they did. They could only wait to be crushed.

Position after Black's 27th move.

BLACK.



WHITE.

Chess Endings. By E. FREEBOROUGH. (Messrs. Kegan Paul & Co.)—This work is a valuable addition to chess literature. Previously to its publication, the only modern treatise on end-games in the English language was that of Horwitz, comprising a collection of positions by Kling and Horwitz. These, however beautiful as problems or semi-problems, were for the most part hardly genuine end-games, the positions being unlikely to occur in actual play. The student was therefore driven to rely on the work of Herr Berger, the great German authority on endings; but at the same time he must have felt the want of something written in his native tongue, and (shall we say ?) a little shorter. This want has been supplied by Mr. Freeborough, who, while of course largely indebted to the above-mentioned treatises, has succeeded in avoiding the defects of both. The book contains about 550 diagrams, of which the respectable proportion of 140 are devoted to King and Pawn Endings—by far the most important in our opinion. These are rightly placed first, in their natural position, not relegated to the end, as in Berger's work. The book, as far as we have examined it, is singularly free from misprints, the bane of Horwitz's work. We have noticed, however, two or three, and give them below, together with some suggested amplifications :—

No. 10.—Note 1. "If 2. . . . P to Kt4ch ; 3. K to Kt3." Add here, "And if then 3. . . . P to R4 ; 4. . . . K to Kt2, as in No. 14." In the same note, after the words "If otherwise to Kt3," a variation might be given : *e.g.*, 2. . . . P to Kt4 ; 3. K to Kt3, P to B5 ; 4. K to Kt4, P to R4ch ; 5. K to B3, and wins.

No. 15.—Here there is an error of some kind. The note implies that Black (with the move) wins by P to B4ch ; White however wins by K to R3.

No. 26.—"White wins through being able to advance his Pawn one or two squares" : *i.e., even without the move.* With the move he wins apart from this privilege by P to Kt3.

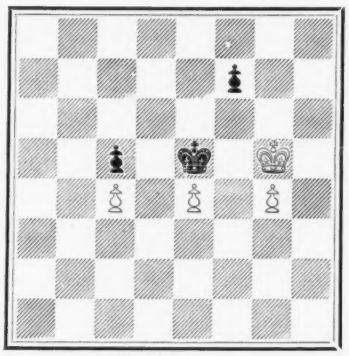
No. 31.—Black should have played 3. . . . P to Kt5, and drawn.

No. 34.—Add perhaps, If 1. . . . K to Q2 ; 2. P to B6.

No. 40.—Should be carried out further, the position, as Mr. Freeborough leaves it, being very instructive, and the win for Black not too obvious.

Position after Black's third move.

BLACK.



WHITE.

White now plays 4. K to R6. Black now wins as follows : 4. . . . K × P ; (K to Q5 only draws. For K to B5 see A.) 5. P to Kt5 (if 5. K to Kt7, P to B4 !). 5. . . . K to B5 ! ; 6. K to R4, K to B4 ; 7. K to R5 !, P to B3 ! ; 8. P × P, K × P and wins.

(A.) The following attempt to win would result in loss. After 4. K to R6, K to B5 ? ; 5. P to Kt5, K to Kt5 ; 6. P to K5 !, K to B4 ; 7. K to Kt7, K to K3 ; 8. K to B8, and wins.

No. 43, last note. Here, too, the analysis might well be continued : *e.g.*, "For if 3. . . . P to Kt5 ; 4. K to Q5 ; and if then both sides queen a Pawn, White wins the Queen in three moves. Again, if 3. . . . K to K3 ; 4. K to K4, K to B2 ; 5. K to Q5, &c.

No. 130.—Here, too, an error has crept in to the last note. 3. . . . P to Kt3 loses for Black by 4. P to KR4, P to Kt4 ; 5. P × P, K to Bsq ; 6. P to Kt6 and wins.

No. 148.—In this position a mate in three moves appears to have been overlooked.

No. 394.—The diagram is incorrect. White can mate in *one* move.

It is hoped that the above notes may be of some slight value to readers of the work, or even to the editor of it himself in the not unlikely event of future editions. It need only be said, in conclusion, that the arrangement of the whole book is lucid, the diagrams abundant and well-placed, and the printing both accurate and excellent.

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